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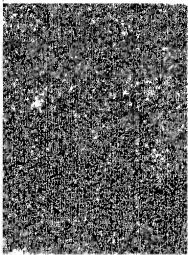
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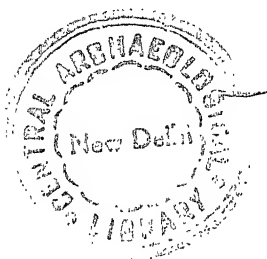
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FIG. 1.—CANOES AT JINJA IN UGANDA, ON THE VICTORIA NYANZA.



FIG. 2.—INTERIOR OF A VICTORIA NYANZA CANOE, JINJA, UGANDA.

# MAN

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## ORIGINAL ARTICLES.

With Plate A.

Africa, East: Technology.

Hornell.

**Indonesian Culture in East Africa.** By James Hornell, F.L.S. With Plate A. 1

In 1926, when at Jinja on the Victoria Nyanza, I had an opportunity to become familiar with the details of the great canoes used by the Bantu natives of that part of the lake. This examination confirmed and strengthened the belief I have long held that the type of these Jinja craft, and of those others seen by Stanley and figured by him in his book "Through the Dark Continent" (vol. I, p. 451), is not of African origin, and is to be referred to the time when the Indonesian colonization of Madagascar was most active.

The Lake canoes, as will be seen by reference to the photographs on plate A, are large, well-built craft, having two broad strakes raised upon the edges of a dugout base. The lower strake flares outwards at a considerable angle, the upper is vertical. The planks are sewn together by means of twine made of strands of raffia fibre, passed several times through holes in the adjacent edges bored by a red-hot iron rod. To prevent leaks, a pad of plantain fibre is applied to the joints on the inner side and this is kept in place by several stems of Elephant grass arranged longitudinally against it, the whole being pressed tightly against the joint by the twine which sews the planks together, and which binds in this caulking material at the same time. Another pad of plantain fibre is placed against the joint on the outside, the pressure necessary to keep the packing tight being obtained by tapping in, slowly and carefully, a narrow strip of tough wood under the twine "stitches" on the outer side. The holes in the edges of the planks through which the twine passes are caulked with vegetable fibre with the aid of a pencil-shaped stick. In spite of these precautions a certain amount of water leaks in; this collects in a bailing hollow in the floor towards the aft end; the second man in front of the steersman is the bailer in a 10-thwart canoe.

The form of the bow and the arrangement of the thwarts are the other outstanding peculiarities of these canoes. The former is bifid, the dugout base,

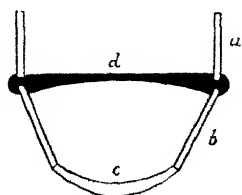


FIG. 1.—DIAGRAMMATIC SECTION THROUGH THE HULL OF A VICTORIA NYANZA CANOE.

the equivalent of a keel in European boats, being prolonged at the fore end into a ram-like projection extending several feet beyond the base of the stem proper. As figured in this note (Pl. A., fig. 2), the ram end of the "keel" is produced to a

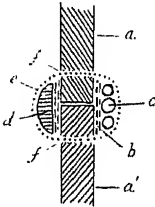


FIG. 2.  
DIAGRAM SHOWING  
THE METHOD OF  
CLOSING THE SEAMS.

considerable distance upwards by means of a long elbow-shaped piece, whereof the horizontal arm is bound to the upper side of the ram, whilst the vertical distal arm towers well above the true stem; the latter scarcely projects above the upper edge of the top strake. The head of the false prow, carved into a knob, with several encircling grooves immediately beneath, is usually decorated with a bunch of feathers rising out of a large mop of fluffy raw cotton, while several strings of glass beads and glass rings hang pendent below. A cord hung with long strips of white cotton cloth is stretched from the summit of the true stem to the decorated head of the false one.

This scheme of decoration of the false prows appears to be a comparatively recent modification of that figured and described both by H. M. Stanley and Sir H. H. Johnston. In their day a pair of horns—of sheep, goat or antelope—rising from a great "beard" of long grass, occupied the position of the feather tuft of the present day, while from the taut line steadying the false prow and connecting it with the true stem head, hung a deep and heavy fringe either of banana filaments or of long grass; Stanley in one place (p. 171) described this fringe as waving like a mane when one of these canoes came charging down upon him "bold and confident, propelled by forty paddlers."

As to the thwarts, they are both massive and numerous, and serve to tie the sides together as well as to furnish seats for the paddlers. In most instances seen they numbered ten, inclusive of the short one on which the steersman sits. Their great peculiarity is that they pass through the sides of the canoe, their ends appearing as prominent bosses on the outside. One of peculiar form, said to be the seat of the headman, is fitted across the bows about a foot aft of the stem; it projects outboard eighteen inches or more on each side, thus giving an impression of the canoe having horns; they are of use when pulling it ashore.

Occasionally a sail is used; when it is, the mast is stepped in a hole in the dugout base, a little aft of the mid-length, and is steadied by being passed through a hole in one of the thwarts.

This is, however, a modern innovation, for Sir Harry Johnston comments on the strange fact that the Baganda never conceived the idea of using sails, although Arab sailing craft had been familiar to them for years ("The Uganda Protectorate," I, p. 660).

Dimensions.—Average length of hull of the larger sizes from stem to stern, 26 feet; beam, 40 inches in one, 48 in. in another; depth, 32 inches. Length of true stem, 42 in.; length of false prow from insertion to further end of elbow, 6 feet; vertical height, 3 ft.

Width of upper strake, 12–14 inches.

„ of lower strake, 16–18 inches.

„ of dugout base, 22 inches.

Fifty years ago far larger canoes were in existence. The largest which Stanley saw measured 72 feet in length, 7 feet 3 inches in breadth, and was 4 feet deep from keel to gunwale. In this canoe there were thirty-two thwarts and, as they paddled double banked, sixty-four paddlers formed the full complement.

The people who build and employ these canoes are of Bantu stock; the names given the respective parts are as follow, viz. :—

Prow = bulamba; stern = bulumba; false prow = lulimi (tongue); projecting "horns" of foremost thwart = gammi; dugout base = kigongo; upper strake = mpero; lower strake = mabasi; thwart = banga.

To find a parallel instance of boats possessing a bifid stem, thwarts passing *through* the sides with their ends showing externally, and prow ornaments of related design, we have to go to the Malay Archipelago. In the ports of Java small coasting vessels having all these peculiarities are common. They are particularly numerous in the island of Madura, as I have good reason to remember, for I was arrested as a spy when photographing some of them. The Madurese lavish much decoration on the prows and in some instances have an ornamental "bowsprit" of no actual use, but possibly homologous with the rope connecting the false and the true stem heads in the Victoria Nyanza canoes; similarly the large wooden horned ornament at the stem is to be correlated to the horns formerly used to decorate the false prow of the African craft. The deck beams pierce the sides and show conspicuously on the outside, the ends being often painted a different colour from that of the rest of the hull; the ram-like projection of the keel is short and stout. In view of these facts and of a number of other considerations, prominent among which is the fact that the common fishing canoe of the east coast of Africa, from Mozambique to Somaliland, though differing in details, is unquestionably derived from the same type as the outriggers of Madagascar, and that this type is known nowhere else than in Java, I can come to no other conclusion than that Indonesian settlements at one time existed upon the east coast of Africa at the time of the Indonesian colonization of Madagascar; further that the origin of the Victoria Nyanza canoes, equally with the double outriggers of the coast, is to be traced to Indonesian culture exercised upon the Bantu tribes of this region by Javanese settlements along the coast—settlements subsequently obliterated in the same way as was that of the Portuguese at Mombasa in the seventeenth century.



FIG. 3.—PROW OF A SMALL COASTING VESSEL OF MADURA, DUTCH EAST INDIES.

In arriving at these conclusions I regret to find myself at variance with Dr. G. Elliot Smith's correlation of this Victoria Nyanza canoe with certain ancient Scandinavian petroglyphs of vessels which appear to possess bifid stems with a horned head upon the outer end of a prolongation of the true stem ("Ancient Mariners" in the *Proc. Belfast Nat. Hist. and Phil. Society*, 1918, and elsewhere). The resemblances emphasised by him are illusory and do not exist. It would appear that Dr. Elliot Smith has been deceived by a certain haziness in Stanley's drawing, for in the figure which he gives purporting to be a copy of this sketch, he has substituted a stout pole in place of the *string* connecting the horned head of the false prow with the true stem head, clearly and correctly described by Stanley at page 71; again, instead of showing the false prow as an elbow-shaped addition to the keel-ram, with the horned head at the upturned extremity, he terminates it abruptly some distance beneath the horned prow head and entirely *unconnected therewith*—treatment which creates a fictitious parallelism between the African and the Scandinavian type.



## DESCRIPTION OF THE ILLUSTRATIONS.

Pl. A, Fig. 1.—Two plank-built canoes at Jinja, on Lake Victoria Nyanza, showing the bifid prow characteristic of these craft, the decoration of the false stem-head, and the protrusion of the thwart ends through the sides.

Pl. A, Fig. 2.—Interior of a similar canoe under construction to show the form of the thwarts and the method of sewing the planks together over canes applied to the caulked joints.

Fig. 3. A small coasting vessel belonging to Madura, the large island lying off the N.E. coast of Java. Note the bifid bow formed by the prolongation of the keel, the projection of the ends of the deck beams (= thwarts) through the sides and the horned ornament at the stern which may be correlated to the horns used to decorate the canoe figured by Stanley; the slender chains with suspended shells which hang from the corners of the broad stem-head may be considered homologues of the strings of beads hung from the false prow in the Jinja canoes.

All photographed by J. Hornell.

*Line Drawings.*

Fig. 1.—Diagrammatic section through the hull of a canoe. *a*, the upper strake; *b*, the lower strake; *c*, the dugout base; *d*, a thwart.

Fig 2.—A diagram showing the method of closing the seams: *a*, upper strake; *a'*, lower strake; *b*, packing of plantain fibre; *c*, three stems of elephant grass; *d*, strip of wood tapped in under *e*, the twine sewing the edges of the strakes together; *f.f*, the holes through the adjacent edges of the two strakes. (Not to scale.)

## Britain: Technology.

Okey.

**The Wicker Bugtrap.** *By T. Okey.*

2

"One could draw up quite a long list of bites which are pardonable and indeed occasions for sympathy. A man (which of course includes woman) may with perfect propriety be bitten by: A gnat; a mosquito; a bee; a wasp; and many other things. On the other hand, he shall not under peril of ostracism be bitten by a \* \* \* \* Still less shall he be bitten by a \* \* \*"—GORDON PHILLIPS.

There can be but few surviving basket-makers who in the late 'fifties or early 'sixties of last century made as apprentices or journeymen the wicker bugtrap in use as part of the domestic furniture of a middle-class house. In the London basket-makers' list of wages (1850) five sizes are specified, ranging from 36 inches by 11 inches to 53 inches by 11 inches. In later lists (1865, 1877 and 1896) one size, 36 inches by 11 inches, alone survives. They disappear from the 1916 list.

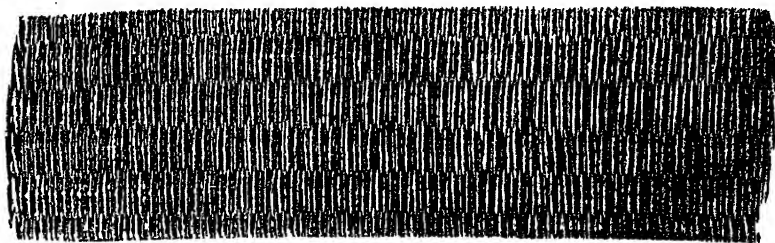


FIG. 1.—WICKER BUGTRAP. FRONT. (Photograph by T. A. G. Strickland.)

Wages ranged from 6*d.* to 9*d.*, and sale prices from 1*s.* 3*d.* to 2*s.* A Grimsby list (1869) specifies a size 42 inches by 11 inches. I can find no record in Midland lists. Bugtraps figure as *Claies aux puces* in French basket-makers' lists. A colleague tells me a specimen was once exposed in a basket-maker's window near the Hay-market, and attracted so much attention and involved so much correspondence that it was speedily withdrawn.

I well remember that in my great-grandfather's house in Whitechapel, and in my father's in Spitalfields, such traps lay in every bedroom—wooden beds—the ancestral *thalami* being formidable four-posters. Examples of these traps must be rare, even if one exist. I have therefore made as an archæological exhibit for our museum at Cambridge a typical trap such as I used to make in the early 'sixties (see Figs. 1 and 2). The implement was placed behind the bolster across the bed, and the

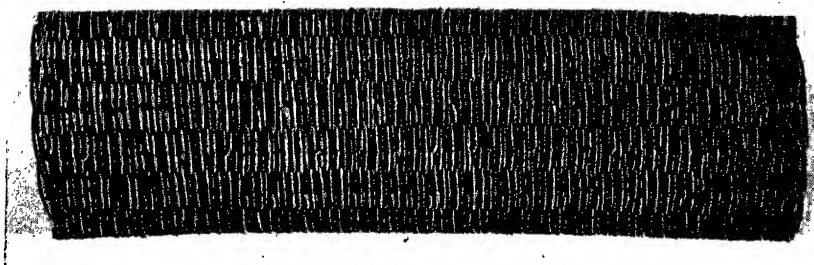


FIG. 2.—WICKER BUGTRAP. BACK. (Photograph by T. A. G. Strickland.)

little anthropophagi, as is their wont, after their nocturnal meal retired to the nearest interstice, admirably provided by the wickerwork, to rest and digest. In the morning the maid or the housewife carried the doomed creatures to the yard or garden, shook them out, crushed them and returned the trap to its place. By this means stray parasites were trapped and a relative immunity obtained.

Thus in middle-class households. Among the common people such zoological species were regarded biologically as related to symbiosis rather than to parasitism, and argued the existence of a beneficent Creator who for poor folk provided a substitute for the periodical bleeding deemed necessary to health by their richer neighbours. A conception of providence not, however, confined to the uneducated. In the "First Steps to Useful Knowledge," published in 1848 by the S.P.C.K., Henry, Mary and Robert are taken by papa and mamma to the seaside, and Henry (the little prig) delights to show his superior knowledge by expounding to the younger children the habits of maritime fauna and flora. Papa, too, contributes and imparts the information that the creation of the leech is due to the extraordinary and providential circumstance that those creatures possess even to their hurt so great a thirst for blood that they may be made useful in checking disease. Even royal palaces were not free from pests. The chairman of the College of Pestology, Mr. A. Moore Hogarth, informs me that the College possess a receipt given by Mr. C. Tiffin, the bug-destroyer to His Majesty King George IV., for destroying bugs in four bedsteads, £2 2s. 0d., in July, 1827. The change of attitude in the present century is exemplified by Mr. Justice Swift's classification of bugs as dangerous insects capable of causing assessable injury.

T. OKEY.

## Britain: Archæology.

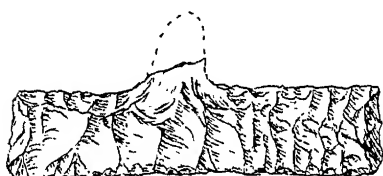
**A Remarkable Flint Implement.** By R. C. C. Clay, F.S.A.

This interesting implement was picked up on the western slope of Harris' Hill, Sutton Mandeville, Wilts.—a portion of the greensand terrace of the Vale of Wardour. It is made from dark, translucent flint, lustrous but unpatinated. It consists of a rectangular body, 50 mm. long, 12 mm. in width and 3 mm. in thickness in the middle, and a stout tang (imperfect) springing from the centre of its back. The back and sides are blunt, while the lower edge is sharp and is obviously the business end of the implement. Both back and front are flaked all

Clay.

3

over, the edge being formed by parallel chipping, finished off by "nibbling." From the same site have come many implements of flint and chert, including fragments of polished celts, and arrowheads of the transverse, leaf-shaped and barbed and tanged varieties. These implements are exactly similar in patination and condition to the specimen under discussion. No parallel to it can be found. One suggestion (a very unsatisfactory one) as to its purpose is that it is a copy in flint of the perforated, hog-backed bronze knife or razor (*see* Evans's "Bronze," fig. 261).



The imitator in flint could not perforate his reproduction, and so he did the next best thing and substituted a tang. Copies in flint and stone of bronze implements are not unknown, and reference may be made to the stone spearhead from Llanelly (*Trans. Carmarthenshire Antiq. Soc.*, XLVI, p. 54), and to the chloromelanite axe from Sept Saints,

near Erdeven, and now in the Carnac Museum. But the difficulty with this suggestion is that bronze hog-backed knives belong to the late Bronze Age, possibly to the overlap of the Bronze and Early Iron Ages, whereas the other implements from this site suppose an early Bronze Age date (*Wilt. Arch. Mag.*, XLIII, pp. 156-162). Furthermore, there is evidence that, in the south of England at any rate, flint implements were not commonly employed during the late Bronze Age. The specimen is now in the Devizes Museum.

R. C. C. CLAY.

## Archæology.

Warren.

**The Study of Comparative Flaking in 1927.** By S. Hazzledine Warren.

4

For many years I have consistently maintained that certain groups of flakings are not prehistoric human industries. Among these one may mention the groups found below the Craggs and the Forest Bed of East Anglia and within the Red Crag at Foxhall, the "Chellean" flakes on the foreshore at Cromer, the "Mousterian" from the Chalky Boulder Clay, and now the "Mousterian" under the low cliff at Rosses Point in Sligo. I do not suggest that these several groups were produced by an identical agency: indeed, I suggest quite the contrary of this. They are by no means identical in detail, yet, in spite of their differences, they are linked together by certain broad characters that they have in common.

They one and all represent a common standard or basis for making the distinction between human and non-human flaking, which I will speak of as the Eolithic standard, even although some may be called "Chellean" or "Mousterian."

Now, if this Eolithic standard leads to an evolutionary sequence of industries, then it is true. But if, when followed out consistently, it leads to confusion which is impossible, then it is false.

It has previously been shown\* that this class of Eolithic flaking is to be found abundantly in certain situations in the Bullhead flint bed which underlies the oldest Tertiaries. But here it is always possible for my opponents to assert that the Bullhead and the disputed flakings are different, although one may incidentally draw attention to the circumstance that they have not yet explained what the difference may be.

I have been waiting in anticipation of a case where my opponents would describe such a group of flakings as a human industry, but where the associated evidences of position would subsequently show that it could not be of this nature.

\* H. Breuil, *L'Anthropologie*, Vol. XXI, 1910, p. 382. S. H. Warren, *Quar. Journ. Geol. Soc.*, Vol. LXXVI, 1921, p. 238.

I think that we now have such a case in the affair of Rosses, and perhaps interested onlookers will pause to consider its import.

These "Mousterian" flakings were alleged to have been found in caves and rock-shelters, but on examination the latter prove to be due to the present-day erosion of the sea under a low cliff, one of these "rock-shelters" being more recent than a beacon-tower which is about fifty years old. Thus the possibility of a prehistoric date for these "Mousterian" flakings is ruled out by the position in which they are found.\*

In fact, the supposed resemblance of the Rosses flakings to the Early Mousterian is very remote, and my opponents, quite correctly, state that the Rosses technique cannot be matched with that of any Mousterian previously known. There is a much closer simulation to a later development of the true Mousterian in the Crag flakings and also equally in the Bullhead series.†

There is another of these groups where there is, actually, good evidence to indicate a post-prehistoric date, although in this case it is less obvious than at Rosses. I refer to the "Chellean" flakes found on the open beach at Cromer. These are by no means all in a highly patinated (ochreous) condition; some are quite fresh and new, while every intermediate stage is well represented. Many different dates of flaking are often to be seen in the same specimen. I have one critical specimen in which the outer face is old and richly patinated, while the inner face carrying the bulb is only slightly patinated. That is to say, some flakes were struck off and the facets remained on the parent block while they became deeply patinated, and then during some long subsequent storm the flake itself was removed, carrying the patinated facets with it.

I have another in which the outer face is only slightly patinated and the inner face quite new, besides several more in which a difference in date between the outer and inner face is unmistakeable. The conditions cannot be explained as being merely unequal patination on the two faces, because several of these specimens possess some later, comparatively unpatinated, facets on the outer face as well as the earlier facets.

I am also assured that some of these Cromer beach flakings that are supposed to have come from under the Forest Bed are made out of "common-shot" flints from the Glacial gravels. Mr. F. N. Haward has a particularly convincing example.

The patination of these "Chellean" flakes is a beach phenomenon, by no means confined to Cromer, and there is no reason for separating the older flakes from those that are fresh and new. Whether this ochreous patination may be due to ferruginous spring-water that is thrown out on the foreshore, or to some organic or other chemical process, does not seriously concern us. It is certainly, from whatever cause, "patchy" in its action, and where the conditions are favourable it is much more rapid than the wearing away of the stones by the waves.

From time to time I am asked if I can give a simple and definite rule for what I believe to be the reliable determination of flakings. Before answering that question I would ask another: Can you give me a simple rule for determining the bones of a rhinoceros from the bones of a hippopotamus?

The answer, in both cases, can only be found in personal judgment, with all its attendant risks of being swayed by prejudice, desire, or insufficient knowledge.

But there is a wide difference between judgment that is trained and has the benefit of accumulated experience behind it and judgment that is untrained. The

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\* R. A. S. Macalister, J. K. Charlesworth, and others, *Nature*, 5th November, 1927.

† The Mousterian simulations among the Crag flakings are well known; some corresponding examples from the Bullhead series are illustrated in *Quar. Jour. Geol. Soc.*, Vol. LXXVI, 1921, p. 246, Figs. 26, 27, 28, 29, 30. I have found others since 1921.

study of comparative flaking is so new that there is no accumulated experience behind it.

Nevertheless, as in the comparative study of bones, so likewise in the comparative study of flakings, I believe that there is a definite method through which the training of judgment can be secured.

To begin at the beginning, in brief outline of the general principles, flint nodules are commonly more or less rounded in shape. In consequence of this, they supply many pieces of plano-convex form as they break up under the disruptive forces of Nature. Now, when such plano-convex pieces are subjected to force acting indiscriminately in all directions, they do not flake indiscriminately in all directions equally, in accordance with the incidence of the force. They tend to flake systematically in certain selective directions and to result in the production of a definite but complex group of effects.

Commencement should be made with small plano-convex pieces under a low force. Then careful observation should be made of the changes in the effects as the force is increased, and as larger pieces are used, and as these are of different proportions of thickness to length and breadth. Then note the further changes as tabular or other forms are employed in place of the plano-convex.

The effects of the interactions of variations in force, form and size are of necessity highly complex, yet they are so surprisingly systematic that they are not nearly so difficult to grasp as might be imagined. Although there is no rule to be followed, there is a relation of cause and effect which can be understood, and that is essential to the understanding of the subject.

I say "surprisingly systematic" advisedly, because due allowance must be made for the irregularities of quality and texture in the flints employed.

The analogy of a mineral is rather illuminating in this connection. The perfect crystal is rather rare than otherwise, yet the crystalline form is the underlying truth that tends to come out so far as circumstances permit.

The study of comparative flaking is a very new subject, and the text-books do not yet present a case for any such essential underlying truth. It is a fair argument that where the text-books are silent there is nothing to be learnt. Perhaps it is also a fair argument that the truth must be proved before it finds its place in the text-books. Whoever will sincerely and adequately put the matter to the test can prove the truth for himself.

I venture to think that it is the lack of any systematic basis to the subject which leads to such mistakes as are brought out by the "Mousterian" of Rosses in a modern situation, or the "Chellean" flakes which are still continuing to be made on the foreshore of Cromer—and of other places besides Cromer.

Time will show what further developments will arise from following what I have referred to as the "Eolithic standard." There are the earlier geological formations, such as the Bunter, which yield good flakings, although they are as yet almost untouched. I have an "Acheulean" (or, perhaps, rather a "Mousterian") form of ovate which is, I think, superior to any one of the Rosses "implements," and this I found in the Permian of Devonshire.

I believe that this prolonged controversy will shortly enter upon a new phase, in which the onlooker will realise that the "Eolithic standard" is beginning to disprove itself, so that he will look round, firstly, to seek the underlying cause, and, secondly, to find the cure in the study of comparative flaking.

This communication purposes only to deal with generalities, but it is intended to be followed by the presentation of further practical evidences.

S. HAZZLEDINE WARREN.

America, Central: Archæology.

Gann.

**Recently Discovered Maya Temples in Yucatan with Date  
Sculptured on Wooden Lintel.***By Thomas Gann, F.R.G.S., F.R.A.I.***5**

Early in 1927, while exploring the dense forests in the south of Yucatan, I came upon a series of large ruined Maya temples never before visited. These were situated in the bush immediately to the west of the Bacalar lagoon, and were completely buried in the primeval forest with which this region is covered.

Seven temples were explored, all of which presented the same remarkable characteristics.

They stood upon the summits of great oblong, truncated cones, covered with cut stone, and almost perpendicular, and were approached by steep narrow stone stairways passing up one side of the substructures. Each temple contained either one or two rooms, varying from 50 to 60 feet in length, 18 to 20 feet in height, and averaging only 3 feet in breadth.

The Maya were greatly restricted in their architectural achievements, and more especially in the width of their rooms, by the fact that they never discovered the principle of the true arch, and had to resort to the clumsy expedient of overlapping courses of masonry, covered at the summit by flat flags, or capstones. This restricted the breadth of their arches to about 16 feet. But rooms of only 3 feet in width—and hardly any of the rooms in these temples exceeded 3 feet, and some did not even reach it—are practically unknown elsewhere in the Maya area. It is impossible that they should have been used as dwelling-places, either for the priests or nobles, as moving about in such a restricted space would have been impossible. Moreover, the back rooms in those temples which possess them must have been in almost complete darkness, and required artificial light both by day and night.

The only conclusion one can arrive at, therefore, is that the temples were used purely for ceremonial purposes, and that such ceremonies as took place in the holy of holies were performed either by artificial light or in the dark.

Each structure was surmounted by a lofty roof comb, most of which had now fallen. The total original heights of these temples were between 100 and 200 feet, from the bases to the summits of the roof combs. The best preserved of them, though one of the smallest in the group, contained two exceedingly important carved lintels made of sapote wood, upon which was inscribed a date.

Its external measurements were 66 feet in length, 22 feet in breadth, and 25 feet in height. It stood upon a substructure precisely similar to the others, but only 52 feet high. The roof comb, a great part of which had fallen, had been, probably, 25 feet high, so that the whole building from the ground level to the top of the roof comb was, originally, approximately 102 feet in height. The temple was almost exactly orientated, its long diameter running north and south. The outside was decorated by alternating broad and narrow oblong sunk panels. Both walls and roof comb had originally been covered with smooth stucco, painted throughout in various colours, and traces of red and green paint were found still adherent to these in two places. On the western side of the building a steep stone stairway, 26 feet wide, extended from the ground level to the only entrance to the temple, situated in the centre of its western *façade*, and opening into the western room. The doorway had completely collapsed, probably owing to the fact that the lintel had been made of sapote wood, which, after centuries of exposure, had gradually rotted away, and at last fallen in under the tremendous superincumbent weight of the roof and roof comb, which it supported. The interior of the temple was divided into two long, narrow rooms, each 57 feet long, 20 feet high and 3 feet wide. The dividing wall between the two chambers was 5 feet thick, and was pierced, near its northern and southern extremities,

by doorways, leading from one chamber to the other. These doorways were 16 feet high by 5 feet wide, and the lintel of each consisted of three squared wooden beams placed side by side. That over the northern doorway had almost completely rotted away, but that over the southern doorway, having been preserved from contact with moisture, was in an excellent state of preservation. On it were carved in low relief two beautiful little oval cartouches, each measuring  $25\frac{1}{2}$  by  $21\frac{1}{2}$  inches. Both were filled with hieroglyphics.

Cartouche A (Fig. 1) records the date of the temple. Glyph 1 records the period 18 kins, or days, and 4 uinals, or months, represented by 3 bars for 15, and 3 dots for 3, on the left of the glyph, and 4 dots for 4, above it. Glyph 2 may represent 2 tuns, or years, but the symbol used for the tun is not one previously known. Glyph 3 undoubtedly represents the day Ahau with the

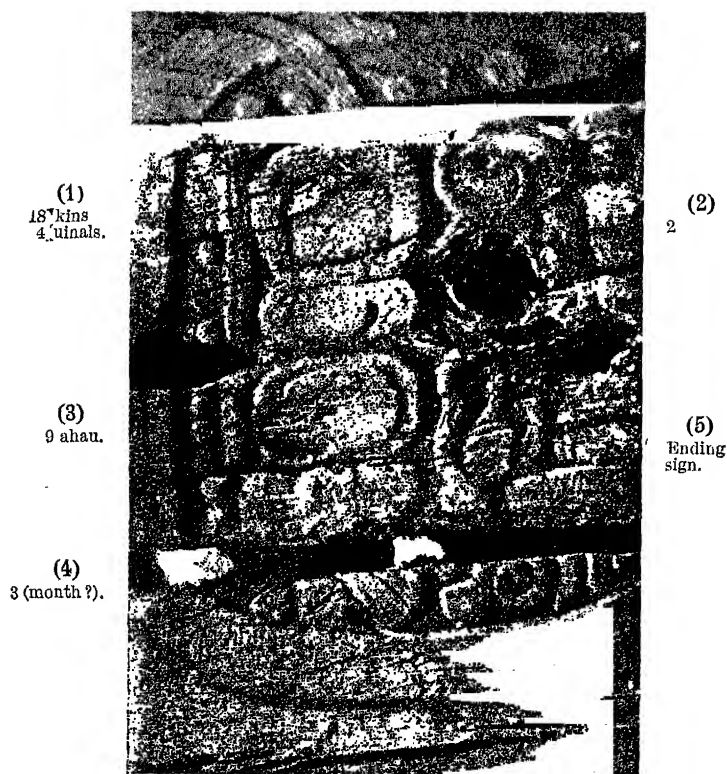


FIG. 1.—CARTOUCHE A.

numerical coefficient 9, *i.e.*, one bar for 5, and 4 dots. Glyph 4 is a month sign, badly obliterated, with the coefficient 3. Glyph 5 shows an ending sign, and the last three glyphs seem to indicate a tun, or year, ending in the day 9 Ahau, 3 (month?).

Now, in Bactun 9, during which practically all important events in Maya history occurred, tun endings in 9 Ahau 3 (month?) occurred nine times, but most of these can be eliminated, either

on the grounds of historical improbability or want of resemblance between the small portion of the month glyph seen in the carving, and the symbols of the Maya months upon which they fall.

Dr. Morley of the Carnegie Institution concludes, and I agree with him, that the date most probably recorded is 9 Ahau, 3 Yax, which corresponds to the Initial Series date 9.15.2.0.0., 9 Ahau, 3 Yax, being 9 Bactuns of 400 years, 15 Katuns of 20 years, 2 Tuns, or years, no Uinals, or months, and 0 Kins, or days, after the starting date of Maya Chronology, corresponding to the year 474 A.D. of our era.

The country of Bakhallal—Hispanicized into Bacalar—where I found these temples, is one of the most interesting spots in the whole Maya area, for it is the first place-name mentioned in the ancient Maya records, known as the Books of



Chilam-Balam, which can still be definitely associated with a known locality. Three of these books record the discovery of Bakhahal Province as occurring in Katun 4 Ahau, which Morley identified with 9.15.0.0.0., 4 Ahau 13 Yax, or A.D. 472.

In view, however, of the fact that last year I discovered a monolith (described in the *Scientific American*) on the Chetumal Bay, within this same province of Bakhahal, bearing the date 9.8.0.0.0., or A.D. 333, I do not consider that it is possible to accept the date A.D. 472 as that of the discovery of the province, which was the first step in the founding of the Maya New Empire.

It has always been considered that, after its first discovery by the Maya, there followed a considerable period, estimated at several centuries, before they founded the earliest cities of the New Empire; yet here we have, in this province of Bakhahal, the very gate of Yucatan, so to speak, for the Maya of the Old Empire, a series of magnificent temples and an enormous aggregation of mounds, platforms, walls, pyramids, and other architectural remains, covering every acre of an extended area, and connoting not only a large population, but occupation over a prolonged period. Amongst all these relics of the past a single hieroglyphic inscription records a date. Now, it can well be imagined that extraordinary interest and importance attaches to the correct reading of this date, for on it may hinge the elucidation of the exact period at which the earliest New Empire founders entered Yucatan, and the mystery of what became of them during the centuries which elapsed between their first arrival, according to their ancient records, in the province of Bakhahal, and the next definite news we have of them, when, centuries later, they founded the great city of the Plumed Serpent, Chichen-Itza, in the northern part of the peninsula. The question naturally arises as to whether these early immigrants did not settle down here to the west of the Bacalar lagoon, and build up the great civilisation whose ruins we contemplated, and not, as has generally been supposed, wander about for centuries seeking a suitable site for a permanent settlement.

I was told by the Indians that these ruins extend for miles to the north, but, unfortunately, I was unable to finish the work of exploration, as I was attacked by malaria and, having forgotten the quinine, was compelled to return to British Honduras for treatment; but, during the field season of 1928, I hope to return to the site, and possibly discover an Initial Series inscription which will date the ruins accurately. I named the site "Tzibanche," which in Maya means "writing on wood."

T. W. GANN.

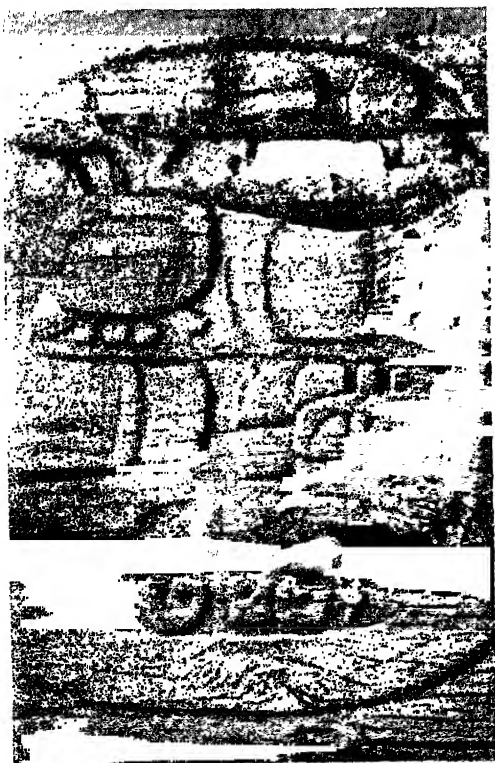


FIG. 2.—CARTOUCHE B.



## Papua: Technology.

Williams.

**A Binandele Drill.** *By F. E. Williams.***6**

Among the Orokaiva, or Binandele-speaking people of the Northern Division of Papua, is found a primitive drilling tool which perhaps has not been fully described. The late W. N. Beaver, in "Papua, Ann. Rpt. 1914/15" (p. 195), gives a photo of the pump drill used by the Orokaiva tribes, and remarks: "Another variety of drill is sometimes used, in which the motive power is supplied by a weight attached to the stick, the end of the latter resting in a shell in the palm of the operator's hand; a movement of the hand imparts a rotary motion."

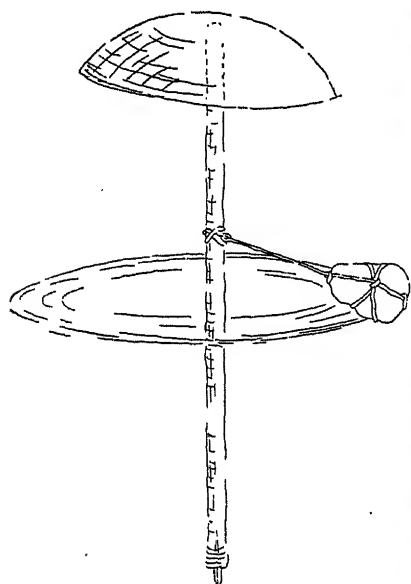


FIG. 1.—BINANDELE DRILL. TOTAL LENGTH ABOUT 18 INCHES.

This latter variety is being supplanted in the Northern Division by the pump drill, which has a wide distribution elsewhere in Papua. In many parts of the Division both methods are known, that of the pump drill being preferred as more effective. But among the Binandele proper and kindred tribes about the Kumusi River the method here described is undoubtedly the original one. It is said to be unknown among the Waria people on the north, and among the "Manigalasi" of the Hydrographers on the south.

The drill is known by several names—*Jigogora*, *Paihona*, or *Hambupari*. It is improvised in a few moments. A quartz (*Kase*) pebble from the river bed is first snapped across the middle. Then, with any pebble as hammer stone, flakes are struck off apparently at random until one of suitable strength and sharpness is found. To expedite the breaking of the quartz the workman may make what can only be described as a labiodental noise, resembling that which a rider makes to encourage his horse, or, perhaps better, the termination of a kiss. This is just one of those imitative noises which are perhaps felt to assist the operation they imitate.

The suitable fragment of quartz is bound into the split end of a stick, say fifteen inches or eighteen inches long. Now a pebble weighing some five or six ounces is attached by a string to a point somewhere near the middle of the stick. The string is three or four inches long. Pressing downward on the top end of the shaft, the workman, with a slight movement of his hand, sets the stone rotating round the shaft. It thus acts as an eccentric fly-wheel, and a continuous rotary motion in

one direction is set up. To make the running smoother, and at the same time to permit of greater pressure, a concave fragment of coconut shell is used as a sort of wide socket or cup for the upper end of the shaft, the hand gripping the convex surface.

The movement is rather slow and steady. The quartz breaks easily and has to be replaced continually. When it is procurable, flint (*Biru*) is substituted. The drill is used especially to bore holes in shell, dogs' teeth, etc.

The Binandele pump drill (*Papai*) is of the ordinary kind. The fly-wheel is usually made of several layers of pottery fragments. A hole is bored through the centre of each with the point of a stone, and the shaft of the drill passed through.

It may be remarked that neither method as practised here would be of any use as a fire-making appliance. The local method of fire-making is by the stick and groove.

F. E. WILLIAMS.

### Assyria: Archæology.

Campbell-Thompson.

**On the Assyrian Mineral *šadanu šabitu* — Magnetic Iron.** By **7**  
R. Campbell-Thompson, M.A., D.Litt., F.S.A

In my "On the Chemistry of the Ancient Assyrians," 1925, p. 120, ff., I tried to show that *tak<sub>K</sub>A* and its compounds represented various forms of iron ores and oxides, and that *tak<sub>K</sub>A.GI.NA.DIB.BA* (= *šadanu šabitu*) was "the hæmatite which 'grasps, attracts.' i.e., the magnetic ore. This is confirmed in an interesting way in Assyrian magic.

Ebeling, in his "Liebeszauber im Alten Orient," published some Assyrian magical rituals for sexual intercourse, and his translations of two of the practical prescriptions to be carried out beforehand are as follow. It will be observed that he does not translate the *šadanu šabitu* mineral at all, and in the case in which he translates the second ingredient as "magnesit-stein" I believe he is incorrect.

(P. 35, l. 15 ff). "Ritual dafür: Mehl (?) des *šadanu-šabitu* Steines, Mehl (?) des Magnesitsteins (?) (AN.BAR) sollst du in Topföl werfen, die Beschwörung 7 mal "hinein rezitieren [der Mann soll seinen Penis, die Frau ihre Vulva einreiben, "sie werden coitieren."

And another, still more indicative:

(P. 43, l. 13). "Ritual dafür: Einen . . . Stein (= *tak<sub>K</sub>šadanu šabitu*) sollst du zerreiben, mit Topföl vermischen, die Beschwörung 7 mal darüber rezitieren, "seinen Nabel (*abunnatu*) einreiben . . . (= *kēm* (?) *parzilli*) zerreiben, mit Topföl "vermischen, die Beschwörung 7 mal darüber rezitieren, den Nabel (*abunnatu*) des "Weibes bestreichen, Mann und Weib werden (dann) [miteinander coitieren]."

First, in this second charm I doubt if the difficult word *abunnatu* means "navel" (see also Christian, "O.L.Z.," xvii, 397, "umbilical cord," and Albright, "R.A.," xvi, 172, "backbone, back, stature," almost certainly wrong). It is used of both man and woman, as Albright saw (as here; of woman, "K.A.R.," 206, ii. 7-8; "C.T.," xxxvii, 45, *passim*; with masculine pronoun, "A.M.," 6, 11, 1, cf. "K.A.R.," 195, rev. 29): 14, 9, 5; Dennefeld, "Geburts.," 210, where it occurs in a line between one containing *papan libbi*, "navel," and another containing *libbi*, "stomach." The word is the crux of the difficult passage in the Old Babylonian (Pennsylvania) tablet of the Gilgamish epic identified by Pöbel and published by Langdon (*Publications of the Univ. of Penn.*, x, No. 3), which, I believe, contains the real reason for the fight between Gilgamish and Enkidu, as I hope to show in my forthcoming edition of the Gilgamish Epic. The idea is, if I am right, that Gilgamish, as ruler of Erech, has a kind of *jus primæ noctis* with any woman after she has entered the city: "he will "sleep with the woman (allotted by) fate; this is the first thing, and then afterwards

" he speaks according to the counsel of god, his decision being according to the action (shape) of his *abunnatu*."

Gilgamesh will seek omens from these means with regard to the future, speaking thereby "according to the counsel of god." Later on, in l. 192, "the couch is spread for the love-rites" (*ana<sup>lu</sup>Išhara*, not for the goddess herself, but for that same kind of rites over which the love-charms in "Liebeszauber," 43, 12, are recited, "By the command of Kanišurra and Išhara have we sent"). This use of the name of a goddess for the domain over which she presides is paralleled by the use of *luNisaba* for "barley," or *luGibil* for "fire."

Finally, it is obvious in this second passage in the "Liebeszauber" *abunnatu* for both the man and the woman replaces [*ri*]*dū* ("penis") and *ūru* ("vulva") for the man and woman respectively in the first. It would thus, from all the evidence, appear to mean *pudenda*.

We can now go on to the use of the drugs in the charms. The ointment for the man consists of the powdered mineral *šadanu šabitu*, which I have suggested is "magnetic iron ore" in oil; for the woman it is "powder (?) of AN.BAB, or *parzilli*, i.e., iron. Clearly here we have a sympathetic use in magic of the magnetic ore in an ointment on the man, with the idea of its power of attracting the iron "powder" on the woman.

R. CAMPBELL-THOMPSON.

#### Africa, East: Technology.

Wayland.

**Barbed Wooden Arrows from Mt. Debasien, Karamojo.** By E. J. Wayland.

8

Early in 1920, when my wife and I first visited Karamojo—then an unadministered territory—I came into possession of a Karamojong throwing-spear, a bow and six barbed wooden arrows. These weapons were taken from a raiding party of that little-known people the Ngikadama of Mt. Debasien, who descended upon the village of Lemoputhi, near the north-east base of the mountain, whereat I had established a food dump. The arrows are of interest; and as nothing like them appears to have been described from this area, the following account may not be out of place.

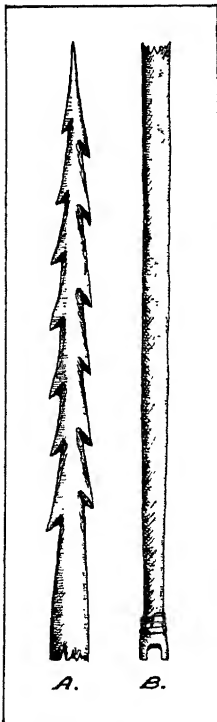


FIG. 1.—POINT AND NOTCHED END OF BARBED WOODEN ARROWS, KARAMOJO.

The arrows on which the barbs are cut are made of straight branches, or slender stems, of a light-coloured hard wood (unidentified). They are circular in section and taper very gently away from the barbed end, the thickest part (average 1 cm.) being immediately behind the proximal barb, and the thinnest part (average 0.6 cm.) just in front of the notched end. In three out of the six examples there is one more barb on one side than on the other. In one instance there are four barbs on one side and five on the other. In two cases there are six on one side and seven on the other. One arrow has four barbs on each side and two have five on each side. In no case are any of the barbs directly opposite each other.

The shafts, which average 133.7 cm. in length (longest = 141.5 cm., shortest = 131.0 cm., including the barbs), are bound in front of the notch, except in one case. Four are bound with vegetable fibre and one with a slender leather thong. The illustrations (Fig. 1), which are drawn to true scale, should make these points clear. A shows the pointed end of one of the unevenly barbed arrows and B the notched end of the one bound with a thin leather thong.

The arrows recall, in some measure, the so-called harpoons of the Magdalenian Age; and for the information of those archaeologists who consider that such harpoons were necessarily used for the capture of fish, it may be as well to record that there are no fish in the Debasien area.

E. J. WAYLAND.

## REVIEWS.

Rhodesia: Archaeology.

Jones.

*The Stone Age in Rhodesia.* By Neville Jones. Oxford University Press, 1926. Pp. xiv, 120. Price 12s. 6d. net.

Africa, as the centre of the great continental ring which forms almost all the rest of the habitable world and as the chief home of the higher anthropods, and that most primitive group of human races—the Ulotrichi—offers problems of the most pressing importance to the Anthropologist.

The stone age is one of these and a work like the present which promises us fresh light from Rhodesia is sure of receiving a warm welcome.

As a foundation for further enquiry we may turn our attention first to the author's account of the human industries and their distribution in space and time.

The "eoliths" are our starting point: they are supposed to be the earliest relics of mankind, but in Rhodesia their chronological horizon does not yet appear to have been definitely established, those found on the ironstone kopjes being, it would seem, superficial. The ancient gravels of the Zambesi, however, are cited as having afforded to the author a stone which he regards rather doubtfully as a rostro-carinate, and this leads to the remark that this sort of implement "has been the centre of a good deal of controversy, but its human origin is now generally admitted by most authorities." It is unfortunate that the author has selected precisely that form of "eolith" on which there is as yet no general agreement; which indeed has been singled out for special exception by those authorities—I need only name for one the Abbé Breuil—who are willing to admit a great number of other eolithic forms into the artefact fold. Of those authorities who reject the whole group of "eoliths" we have Professor Boule, Sir W. B. Dawkins, Professor Macalister and Mr. Hazzledine Warren. The last word has still to be said on this question.

Let us proceed to safer ground. The Palæolithic succession, as shown by the author's observations, begins with the Chellean, represented by bouchers which occur in the gravels of an ancient river-terrace in the valley of the river Unguza at Sawmills. The height of this terrace

above the river is not given, nor in the instructions to beginners on p. 17 is any reference made to the importance of determining, wherever possible, this essential datum.

A scale is attached to the section (Fig. 11 on p. 51), but whether it applies to the vertical as well as the horizontal measurements is not stated; if it does, then the terrace will be situated at about 103 to 131 feet above the river and thus at about the same height as the Chellean gravels of England, France, Egypt and Mesopotamia—a very remarkable coincidence.

Whether any other Lower Palæolithic industry occurs at Sawmills I cannot gather from the description. We turn, therefore, to the now famous locality of Taungs: in the north of Cape Province, where the author has discovered, in gravel of later date than the Chellean, abundant implements of Acheulean type and, further, in a definite stratum overlying this gravel, other implements which are characteristically Mousterian.

The author's comment that this observation is not of sufficient importance to establish the existence of a Mousterian culture in South Africa is rather enigmatical. Mousterian implements, including tortoise-cores, are met with elsewhere in South Africa—in particular in gravels of the Zambesi—which have afforded a Levallois flake to Mr. Henry Balfour. The author's discovery of a Mousterian deposit overlying an Acheulean horizon proves that this industry occupies a perfectly normal position in the chronological succession. If elsewhere Mousterian and Aurignacian implements occur mingled together it must be as a result of some subsequent disturbance.

We are thus assured of the existence in Southern Africa of the three recognised industries of the Lower Palæolithic age and of their homotaxial equivalence with those of that region which I have ventured to call "Chellesia."

For the Upper Palæolithic we must return to Sawmills, where we find a gravel, lying lower than and more recent than the Chellean, which has yielded a "number of beautiful implements" of "distinctly Aurignacian facies." These are not described in detail and the illustra-

tions representing them are not definite enough to enable the reader to form an independent judgment—this is true indeed of most of the illustrations in this work. The drawing referred to as No. 26, Fig. 13, does not agree with the description, and the "typical Aurignacian burins" of Fig. 14 do not show the burin facet. Associated with the distinctly Aurignacian forms are some crescents which suggest the Capsian. The author, however, is careful to state that he does not suggest the existence of any intimate connection between the Capsian and Sawmills industries, though he considers that a common origin is strongly indicated.

In South Africa the existence of Aurignacian forms has long been known, good figures of several types were given by Mr. Johnson from Orangia, and others from Cape Colony have recently been described and figured by Mr. A. J. H. Goodwin, who distinguishes two cultures among them, one of larger forms—the Smithfield culture, the other of microlithic implements—the Wilton culture; he courageously assigns them both to the Capsian and, indeed, to its upper division. The big bone needles and perforated roundels of ostrich eggshell which accompany the Upper Capsian of Spain are not mentioned by Mr. Goodwin, but they were found by Mr. Johnson in some of the Bushmen's kitchen-middens, which afforded him his flint implements. The roundels are the well-known Bushman's beads, which, together with implements of Aurignacian facies, survived into modern times. Thus it would seem that Mr. Goodwin's views are so far justified, since an Aurignacian-like industry seems to have prevailed uninterruptedly in South Africa throughout the whole of Upper Palæolithic time and even later.

Here we are at once confronted with the problem of the so-called "Solutrean" implements of the Still Bay culture which, according to Mr. Goodwin, runs along a part of the south coast and up the east coast of South Africa as far as Swaziland, thence bending west across the Transvaal and South Rhodesia it ends at a single locality at Okahandja in South-west Africa. It is also met with at the mouth of the Congo.

Although the best examples of this industry are far from attaining that remarkable degree of excellence which distinguishes the true Solutrean of France, yet they certainly exhibit the same kind of flaking. This, however, is not all that is required to entitle them to a place in the Solutrean; so far as can be judged from existing descriptions they do not present that variety of forms which are

associated in the Solutrean assemblage; and they do not seem to have been found on a definite chronological horizon.

The suggestion that the Still Bay industry is a local and logical development of the Mousterian seems open to question. The author assumes that fluting or channelled retouch was obtained by pressure and I do not disagree with him. Indeed the fact that this flaking is extremely characteristic of recent American flint implements and that observers have actually witnessed and described the fashioning of these implements by pressure exerted on a bone or horn point would seem to be conclusive. But the secret of the art appears to be lost. The first worker in flint who produced channelled flaking had made a discovery, possibly to his great surprise, like Prometheus, of whom Bacon remarked that on striking the flint he was probably more astonished at the spark than expecting it.

In Europe the Solutrean industry of Solutrean age is narrowly restricted in its distribution both in space and time; but implements bearing its characteristic channelled flaking are found over wide areas of the earth's surface, though not of Solutrean age. They are common in both the Americas, where they range from an unknown but not very remote past down to late historic times; and they are still fashioned by the aborigines of Australia at the present day. We meet with them again in Egypt, where indeed the Solutrean technique reached its culmination in the most perfect examples of this art. Here it ranges in time from the Neolithic into the Bronze age.

But at least as early as the Neolithic and even perhaps earlier still—though of this there is no evidence—men had learnt to hollow out boats from the trunks of trees, and to navigate the Ocean in dug-out canoes. Thus the Melanesians and Polynesians, starting from the south-eastern corner of Asia, have populated the islands of the Pacific and contributed to the populations of both Americas. Nor has the Indian Ocean been without its sea-faring explorers.

Since Lapique first broke ground in 1906 our knowledge on this subject has slowly but steadily increased, with a sudden accession in the last few years due to the remarkable investigations of Professor Rivet, the latest of whose contributions has only been given to the world during the current year.\*

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\* In commenting on the remarkable resemblance between a remarkably odd custom, common to Australia and North America, I wrote: "When a custom is thus widely

We are thus provided with a clue to the anachronous position of the Solutrean technique over so large an area of the globe, including perhaps South Africa itself. At the same time it must be admitted that the Solutrean technique must have persisted somewhere throughout the Magdalenian age to provide a source for its survival in Egypt. We look to future explorations for the discovery of this unknown asylum.

The preceding explanation is, however, not the only one which may be offered in explanation of the Still Bay Culture. Von Luschan, in one of his latest publications, has brought forward much evidence in favour of a wandering of Hamitic people which began a long time ago and, starting from the north-east of Africa, proceeded southwards till it reached the southern point of Africa. Some of these Hamites are still to be met with in the south, especially among the Bantus. There is no evidence, however, to show that the Solutrean technique was introduced by these wanderers and the distribution of the Still Bay industry is suggestive of introduction from the sea.

Of the skeletal remains of Palæolithic man in Rhodesia we know, unfortunately, very little. The Bushmen and related tribes may well be survivors of the Upper Palæolithic, but as yet we cannot point to any fossil representative of the Lower Palæolithic age. The Rhodesian skull by its general resemblance to the Neandertal might suggest a Mousterian horizon, but it is distinguished by important differences which cannot be overlooked—some of its features are more primitive, some less so, and others peculiar; the brain, however, as we are assured by Professor Elliot Smith, is decidedly inferior. The abundant fauna—represented by bones in the same state of preservation as the skull and closely associated with it—is composed of species all of which are still living, with the single exception of a species of rhinoceros, which, as alleged by Dr. Broom, is

“but discontinuously distributed we may conclude that it must be very ancient.” This called forth the following criticism by my friend, Professor Elliot Smith: “But the custom of using steam engines and wax matches, and, in fact, all the modern appliances of civilisation are widely but discontinuously distributed.” Since steam engines, as a means of distribution, were not in existence under the circumstances I contemplated, this objection seemed to me rather inapt, but the remarkable revelations on oceanic migrations which we owe to Professor Rivet have, I must confess, now rendered it completely apposite; they also afford unexpected strength to Professor Elliot Smith’s views in general on dispersion.

extinct. Sir Arthur Keith, as quoted by the author, remarks “Time will show that Rhodesian man represents a Pliocene stage in our evolution.” Let us hope that Time will not procrastinate. Another assertion, drawn from the same source, is that the Bushman never lived in Europe. Professor Paul Sarasin, who is not of this opinion, has lately drawn attention to an anatomical peculiarity which distinguishes many of the Spanish representations of Bushman-like figures: this is the normal attitude of the penis, which is commonly shown as projecting almost horizontally, and he shows by illustrations how marked a character this is in the existing Bushmen. I am the more interested in this comparison as I had long ago been impressed by it myself and commented on it to some of my friends, though I refrained from publication.

In discussing the Bushman art the author attributes to me the opinion that the Bushmen painted simply for the love of doing so, and adds that I base this opinion largely on the statements of Hahn.

I must confess to have read this with no little astonishment. It is true that I commenced a rather long argument with a reference to Hahn, since, in reviewing evidence, the first place is very properly given to the testimony of eye-witnesses who have lived among the people they describe. But I at once proceeded to show that some of the paintings are *historical*, and followed this by the statement that others “are of a very different character, which I cannot help regarding as truly *mythical*.” Of these I gave instances—one a painting with a mythical interpretation by Stow, another with a mythical meaning which was expounded by several old Bushmen to Orpen, and a third, of which I ventured a mythical interpretation myself.

Some of the Bushmen pictures represent dances and I thought to clinch matters by quoting the assertion that all the Bushmen dances were *religious*. “They are to us what prayers are to you” was once said by a Bushman.

I am quite sure the author had no intention to impose upon me an opinion so directly contrary to that which I hold and have expressed, but I cannot help thinking that his reading of the works he quotes must have been rather desultory.

This may also be the explanation of such statements as that in Europe Palæolithic implements are invariably made of flint, that industries can be classified into flake and core industries, that the working of bone began in the Solutrean, and that the burin is a boring implement.

The author will be able to correct these little slips when fresh discoveries call for a second edition. May we not have long to wait.

W. J. SOLLAS.

October 1927.

#### Ethnology.

Taylor.

*Environment and Race. A Study of the Evolution, Migration, Settlement and Status of the Races of Man.* By Griffith Taylor. Oxford University Press. London: Humphrey Milford. 1927. pp. xvi + 354 (135 × 215 mm.). Numerous illustrations and diagrams. Price 21s.

Professor Griffith Taylor's book may be considered as falling into two parts. In the first he considers the evolution of man in relation to his geographic environment, and in the second he discusses the future of the white race, with special reference to Australia. The subject is a big one and one of great complexity, and is all the more difficult because to deal with it satisfactorily needs both geographical and anatomical training, and it is impossible in a short review to do more than touch on a few points, some general, some particular. In the first place, Professor Taylor gives the impression that he believes racial types spread more or less in tribes, e.g., p. 227: "Another Ice Age commences and the same phenomena (i.e., the evolution of a higher race) are repeated. This second Race (B) migrates south. It probably now consists of much larger tribes, who have little difficulty in driving the unprogressive and unchanged portion of Race (A) farther into the tropics. . . ." Sir Arthur Keith has suggested—and there seems no reason to doubt his suggestions—that migrations take place not by marauding bands but rather by a gradual extension of the dominant type. Secondly, Professor Taylor takes indices, cephalic, nasal, etc., and treats them as if they were of absolute value, without regarding the variations from the mean which occur; e.g., p. 155, under Nordic and Teuton (Cephalic index, 77-80) he includes "Type Greek." Apart from the fact that such scanty evidence as we have suggests the average Greek cephalic index is about 81, an examination of any long series of Greek skulls will show that the Greek cephalic index is extraordinarily variable, because the average conceals a racial mixture of dolichocephals and brachycephals, the mesocephalic index being illusory. He appears to suggest that the main racial element in England is "Saxon," but a type hardly distinguishable from the modern Englishman, except in such matters as teeth, was living in England long before Saxon times. He notes with apparent surprise the distribution of the nasal index in America. In this connection and in

others Professor Taylor might well have consulted Professor Thomson's work on the effect of environment on the human form, but no reference is made to the latter's work at all. But it is easy to criticise, and detailed criticisms fail to do justice to what is a most interesting and suggestive work, especially on the geographical side, and one which is full of illuminating theories. The student, however, into whose hands it may fall would do well to read in conjunction with the book Dr. Haddon's "Races of Man" and Sir Arthur Keith's "Antiquity of Man," in which he will find a somewhat different interpretation of the data.

L. H. D. B.

#### Anthropology.

Ploss: Bartels: Reitzenstein.

*Das Weib in der Natur- und Völkerkunde. Anthropologische Studien von weiland Dr. Heinrich Ploss und Dr. Max und Paul Bartels.* Neu bearbeitet und herausgegeben von Ferd. Freiherr v. Reitzenstein. Elfte Auflage. 3 vols and index vol. Berlin: Neufeld und Henius. 1927. Pp. xi + 790; viii + 866; vi + 571. Price 125 Marks.

The first edition of this work was published over forty years ago, and in this, the eleventh, edition the material collected by the late Ploss and Bartels has been considerably augmented and thoroughly revised by Baron Reitzenstein.

The work is now published in four volumes, of which one consists of a very full author and subject index, compiled by Dr. H. Schweinfurth. The first volume is divided into two parts, of which the first consists of a discussion of woman considered simply as a physical organism. Under this aspect the female body is dealt with in some detail and, bearing on the same subject, the various ideals of feminine beauty are considered. The psychological side of woman's nature is then examined and in each case the anthropological significance of the facts is not neglected. Thus, in dealing with the ideals of beauty, tattooing and the various bodily mutilations and deformations are discussed, although the contributions of Jousseume to our knowledge of female infibulation are not mentioned. In the second part the social position of woman is dealt with as it is seen in different parts of the world. This section might have been amplified with advantage, since in some respects the material is more sketchy than seems necessary. Thus, in dealing with ancient America, only a few lines are devoted to Mexico, Seler-Sach's book on the life of woman among the Aztecs not appearing in the bibliography or being mentioned in the text. The latter half of the same section



is devoted to female adolescence, a special chapter being given to a discussion of menstruation and initiation ceremonies.

The second volume opens with a long account of the sexual life of woman from adolescence to motherhood. The treatment of virginity is good, although, perhaps, too much reliance has been placed upon Freydier's "Plaidoyer" as evidence for the girdles of chastity. The remainder of the book deals with obstetrics and constitutes a valuable addition to the already existing treatises of Englemann and Witkowski, although the photographs of labour might have been better selected. The third volume continues the treatment of childbirth and, in addition, has sections dealing with woman in such diverse rôles as mother, widow and witch, the last part, however, not being given the treatment it deserved. Thus, Miss Murray's admirable volume on the witch cult finds no place in the bibliography, and, indeed, the section is too short to be of any practical service.

As an encyclopædia of Woman considered from every angle these volumes are probably unique. They constitute a source of reference to which recourse will undoubtedly be made when any doubt arises upon certain specific points. The work does not pretend to be complete. It is merely a convenient summary of the most important facts drawn from a great variety of authorities and supplemented with over a thousand illustrations which form a running commentary to the text. It is true that some of the photographs appear unnecessary, but, generally speaking, Baron Reitzenstein has succeeded in his task, and the present edition will easily maintain the reputation which the former ones have deservedly held. E. J. DINGWALL.

Switzerland: Archæology. Reinert.

*Die Jungere Steinzeit der Schweiz.*

By H. Reinert. Augsburg: Dr. Benno Filser Verlag. 1926. 288 pp., 8 folder maps and 95 illustrations.

This is an interesting attempt to synthesise the facts for the lake-dwellings

culture of Switzerland, to give it its traditional name, and is based upon much observation of museum collections and in the field. The author's main views are that the so-called "lake-dwellings" were land rather than water settlements, that these settlements represent a fusion of Northern and Western culture-elements at the end of the Neolithic, and that they are related to a southward spread from what is now Germany to North Italy. This dating of the Swiss lake-culture will, it is hoped, receive general support, as it has now been urged by several of those who have given thought to the question. Reinert's plea for reconsideration of F. Keller's reconstructions is interesting, and one could wish that some other experienced student, like Dr. Viollier, had contributed to this book a note giving his opinion. Reinert finds only in Schötz I (in Wauwiler Moos) a real indication of houses on platforms, and even here each house has its own separate platform. In many other cases the author thinks the wooden buildings were on the surface of the ground. He also accepts the view of Gams and Nordhagen, and of Gradmann, that the Swiss dwellings belong to a very dry period about the end of the Neolithic Age of Western Europe.

The author further distinguishes in pottery and stone axes between Western and Northern forms, and is of the opinion that the two influences met and mingled in Switzerland to form the characteristic lake-side civilisation that Keller described. This civilisation spread in due course into the Alpine region and through the passes, and flourished for a while until cooling of climate at the end of the Bronze Age brought extensive changes. The parallelism between the changes that Reinert infers in the climate of Switzerland and those which have been inferred for S. Scandinavia are notable.

The book has tables of finds of (a) the wild and domestic animals, (b) pottery of various types and associations, (c) stone axes, and (d) axe hammers, and a considerable bibliography is added. H. J. F.

## CORRESPONDENCE.

Religion.

### Phallic Offerings.

To the Editor of MAN.

SIR,—In his interesting article (MAN, 1926, 52), "Phallic Offerings to Hathor," Mr. G. D. Hornblower says, concerning the wooden phalli found at the Deir el Bahari shrine: "There can be little doubt that [they] (are offerings to) Hathor as a fertility goddess and are symbols of prayers for the bearing of children with perhaps [an] aphrodisiac

Thomas.

"intention. . . . They cannot be *ex voto* offerings for cures . . . for then models of other parts of the . . . body would have been found: nor are traces of such offerings found elsewhere in ancient Egyptian remains." But even if some of the "votive ears" are conclusively proved to be symbolical of the ear of the listening god, it does not follow that all such are, and the same applies to "votive eyes" painted on plaques. As regards the phalli as votive offerings for cures, the



example of the phallic rite which persisted in the Roman Church at Isernia in Calabria until 1780 affords a case where such offerings were offered by women apparently with a view of obtaining a cure—of impotence in their husbands. A full account of the amazing ceremony is given in Sir W. Hamilton's "Worship of Priapus: an Account of the Worship of St. Cosmo and Damiano . . . at Isernia," written in 1783 from the account of a trustworthy Italian eyewitness. An enormous concourse, chiefly of women, attended the festival of these saints in February. Waxen phalli (emblems of other parts of the body were in a negligible minority) were hawked round in baskets, and when purchased were presented to a canon of the church seated at a table, with a prayer or expression of thanks and a gift of money. The nature of some of the prayers, and of the emblems selected ("*e questo che osservai*") left no doubt as to the object of the offering. At an adjoining table requests and payment for masses were registered by another canon. There are other aspects of the festival which, though kindred to the subject, need not be considered here. The sale of these indelicate emblems was forbidden when the opening of communications brought the region and festival into wider notice. The town was practically obliterated by an earthquake in 1805. Examples of the phalli and the original Italian letters are in the British Museum.

The survival of even more definitely phallic rites, in connection with priapic saints in France, well into the sixteenth century may be mentioned: St. Pothinus (Foutin) being the best known (St. Foutin de Varailles and de Cruas), St. Guerlichon (Greluchon), St. Gilles, St. René, St. Regnauds (these from association with *reins*). The phalli were anointed with wine, or scraped (they were usually of wood and renewable) and soaked in water, the resulting *vinaigre* or the decoction being used to procure fertility. The subject is summarised in Hasting's Dictionary of Religion and Ethics. A full account, with full references to these and other relatively modern examples (and to modern substitutes in the shape of church-door bolts, etc.) will be found in J. A. Dulaure's "Des Divinités génératrices ou Culte du Phallus chez les Anciens et Modernes." Paris, 1805.

2. Lefebure in the Proceedings Soc. Bibl. Arch., 1891 (p. 333 f.) gives philological reasons for considering the Egyptian *menat* amulet (health, joy, life) as a phallic emblem (a form suggested by the shape of the necklace-counterpoise from which it is derived). He suggests that in this form it was bottle-

shaped rather than flat, and probably contained portions of the life-giving kidneys and genitals of the sacrificed bull. The accompanying photograph from the tomb of Sennofer at Thebes (Fig. 1) seems to bear out Lefebure's contention as to its phallic nature. The object is shown with the necklace attached, and a sistrum in the hand of Merit, Sennofer's wife. Another example in the same tomb is an unmistakable phallus, and is also attached to a collar (v. Colin Campbell's "The Gardener's Tomb at Thebes," p. 25). On philological grounds, also, Lefebure shows that the *Sam* (union) amulet

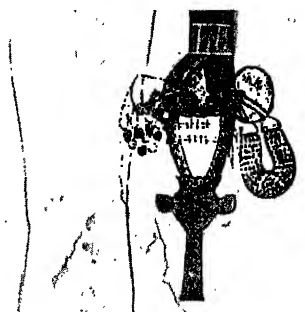


FIG. 1.



comes into the same category. In Petrie's "Amulets," examples of the

*nefer*



are given which are hardly distinguishable from the *Sam*. I am, etc..

ERNEST S. THOMAS.

Britain: Agriculture.

Myres.

### Terraced Cultivation in Berkshire.

To the Editor of Man.

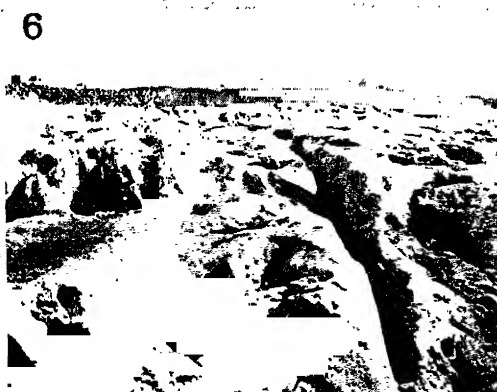
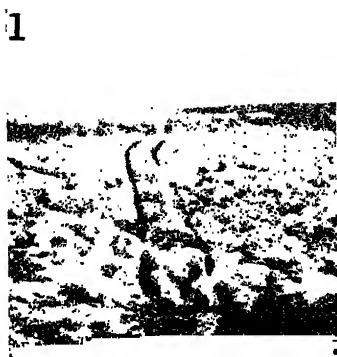
14

SIR,—I was not aware till I read Mr. Perry's letter on "Terraced Cultivation in Cornwall" (MAN, 1927, 166) that even he regarded mere "proximity" as "significant" of connection in origin. In my garden on Hinksey Hill, near Oxford, I have two "terraces," both cultivated, the upper supported by a congeries of loose stones, the other quite devoid of this. In "close proximity"—a few yards only—there are (1) an early iron age settlement, (2) a Roman building, (3) a Victorian country house. If Mr. Perry's argument holds, it "suggests" very strongly that the builders of all three of these monuments also built both terraces, *prope hoc, ergo propter hoc*. I happen, however, to be able to prove that one of the terraces is post-Roman and pre-Victorian, and that the other can only have begun to be formed after the first-named had ceased to have any connection with agriculture.

Yours faithfully,

JOHN L. MYRES.





FIGS. 1-4, 6, 7.—SAN PAUL TAT TARGIA, NAXXAR GAP. FIG. 5.—TA FRATTITA, BIN GEMMA.

## ORIGINAL ARTICLES.

With Plate B.

Malta: Archæology.

Murray.

**The Cart-Ruts of Malta.** *By Miss M. A. Murray. With Plate B.*

15

The ancient wheel-tracks which are found in many parts of Malta have long been of interest to archæologists. As they are being rapidly destroyed—owing to road-making, increase in the area of cultivation, and other causes—it seems worth while to record at least a few of them. Professor Zammit and Commodore Clark Hall hope to make a complete map, from air-photographs, of all the known tracks on the island. This paper must, therefore, be considered only as a preliminary introduction to the study of the subject.

The first point to be considered is whether the ruts are natural or artificial. It is, of course, well known that parallel fissures often occur in limestone, and, if these ruts were only straight lines of varying gauges, there might be considerable doubt as to their origin. But they often curve, and when they do so the distance between the two parallel lines remains the same; in other words, a pair of tracks are always equidistant throughout their length, whether straight or curving. The gauge is also fairly constant, being rather wider than a modern Maltese cart.

The depth of the ruts is not very great: one of the deepest is seen in fig. 2. The photograph shows the section of the cutting for the footpath and steps which join two of the zigzags on the Military Road near Nashar (Naxxar). The rut is about a foot deep.

The archæological evidence for the human origin of the tracks is fairly strong. In ancient Greece such ruts were cut in the rocky slopes of hills for the passage of wheeled traffic, which could not otherwise surmount the uneven surface. (The kind of surface to be traversed is seen in almost all the photographs). Caillemer, in his description of ancient Greek roads, speaks of these tracks as “Ornières artificielles, profondes de quelques centimètres.” He states that the Greeks “creusaient pour les roues des rainures qu’ils nivelaient avec grand soin. Entre les deux rainures, lorsque le sol était trop raboteux ou trop inégal, on répandait du sable ou du gravier.”\* The ruts were probably cut quite shallow and were deepened by wear. “Pour remédier à l’inconvenient des rencontres de chars, il suffisait d’établir deux voies parallèles, ou même, en se contentant d’une voie unique, de disposer de place en place des courbes d’évitement.”† The parallel pairs of ruts are a common phenomenon in Malta, but the curves for passing do not occur in any of those which I have seen. Caillemer gives instances of such artificial cart-tracks in Italy and in the south of France.

There seems to have been a network of these roads over the whole island. The best examples now remaining are on the rocky slope near the Nashar (Naxxar) Gap, close to San Paul tat Targia, down which the new Military Road has been constructed. Another group is at Ta Frattita on the west side of the Bin Gemma hills. Short lengths of such roads are often found in connection with megalithic monuments, apparently leading directly to the monument, as at Santa Sfia and Santa Maria tal Bakkari (see my “Excavations in Malta,” Part II, Pl. XXXIII). In each of these cases the remainder of the road has been obliterated by modern alterations, such as the making of fields, construction of metalled roads, building of houses, and so on.

The age of these ancient roads seems to be indicated not only by their connection with the megaliths, but by the fact that they were made when the

\* Caillemer: “Les voies à rainures chez les anciens.” *Congrès archéologique de France*, 1870, p. 280. See also Daremberg et Saglio: “Via,” vol. v., p. 780.

† Caillemer, pp. 277–289.

configuration of the island was different from its present condition. At St. George's Bay near Birzebuggia a cart-track crosses a little spit of land which juts out into the Bay. Before the houses and Marina were built, this track could be traced on each side of the Bay. It is evident that at one time the sea had not advanced so far as it has now and that there was a road across the valley. (For photograph see "Excavations in Malta," Part II, Pl. XXXIII). Tracks are also found leading to the edge of the cliffs, where they end abruptly owing to a fall of rock into the sea. On the south side the island is continually losing by the breaking down of the rocks and cliffs. (The temple of Shrobb-in-Ghagin is rapidly disappearing in this way; only a small part of it still remains).

In considering the series of photographs given here, Figs. 1-4 are perhaps the most convincing; Figs. 1, 6, and 7 show the broken type of surface, with small fissures at every angle, in contrast with the straight or evenly curving lines of the tracks; these photographs also show the rugged uneven surface between the ruts where the traction animal must have walked, the hollows, according to Caillemier, having been filled up with stones and sand. Such a surface would have given the animal a good foothold; at the present day on rock-cut country roads, where cart tracks are formed nearly as deep as the ancient ones, the middle of the road is deeply scored across horizontally to prevent the animal from slipping. The modern cart-wheel is unusually large, and this was probably the case anciently; the body of the cart would then be lifted well over the high part in the centre of the road.

A map of the tracks will give the centres of population in early, possibly neolithic, times; and will throw light on many of the archæological problems connected with Malta.

There remains a tradition in the island that the tracks were made for "a boat which went on wheels," a kind of *via sacra*. The tradition is, perhaps, the origin of Father Magri's theory that the motive power for the ancient vehicles was sails. But, as Caillemier has noted in the Greek examples, there would be no difficulty for an animal to draw a cart along these artificial ruts when the surface on which the animal walked was made more or less even.

Like almost every country, Malta has retained little memory of her ancient past, and on this particular point there is but the one tradition already mentioned. A great change came over the island in Roman times. The Romans were distinguished from all other nations by their ability in road-making, and the primitive roads of Malta were probably superseded then. It is perhaps significant that Maltese folk-memory and tradition date from St. Paul's shipwreck during the height of the Roman domination.

M. A. MURRAY.

## Archæology.

Burchell.

**Mr. Warren's Views on Flaking.** By J. P. T. Burchell, F.S.A.

I would wish to comment on Mr. Warren's note on "The Study of Comparative Flaking in 1927," recently published in MAN (1928, 4). This note deals with a number of flint implements of different ages and denies the human origin of the limestone specimens found by me in Sligo, Ireland. In the first place I notice that Mr. Warren makes a claim of consistency in regard to his attitude towards prehistoric archæology. The following account of his methods in connection with my Irish discovery is, therefore, not without interest.

On two occasions Mr. Warren paid visits to my house and subjected the Irish specimens to a prolonged and detailed scrutiny. Following upon the first visit he expressed himself definitely as of the opinion that these specimens were to be referred either to the work of palæolithic man or to that of a modern quarryman. A few weeks later, on the second visit, he claimed that the latter alternative was

the true solution. It will thus be seen that Mr. Warren, in the clearest manner, regarded the Sligo material as of human origin. Now, however, he does not hesitate to make the following statement:—

“ I have been waiting in anticipation of a case where my opponents  
“ would describe such a group of flakings as a human industry, but where  
“ the associated evidences of position would subsequently show that it  
“ could not be of this nature. I think that we now have such a case in  
“ the affair of Rosses. . . . ”

This sudden and unexplained change in his opinion upon this question hardly, I think, supports his claim to consistency. In fact, to use his own words, I would ask in regard to this, that “ interested onlookers will pause to consider its import.”

I would also draw attention to Mr. Warren's statement that some of the Sligo specimens were found in caves. This, like his claim to consistency, is, however, incorrect. I have never stated that any of these specimens were found in a cave. On the other hand, I claimed, and I still do so, that I discovered over one hundred unrolled flakes and implements, of indubitable human manufacture, in a collapsed rock-shelter at Rosses Point. The finding of the comparatively few specimens incorporated with the beach material at Coney Island, in view of the fact that I could discover no implementiferous deposit in the island, induced me, temporarily, to put aside my first opinion that these specimens were the work of man. But upon making a close examination of the shore after heavy storms, an examination which failed completely to reveal any newly-fractured pieces of rock, and the subsequent discovery of the same type of implements, unrolled, at Rosses Point, compelled me to abandon the idea of the flaking of these specimens by natural means, and to revert to my original opinion about them. Further, upon making a close scrutiny of the flake-scars and the method of manufacture of the Rosses Point and other material, it became abundantly clear that this material could be none other than the work of man.

In claiming that “ my opponents state that the Rosses technique cannot be “ matched with that of any Mousterian previously known ” Mr. Warren is once more misleading his readers. What Mr. Reid Moir and I said was that the technique of the implements found at Rosses Point was of true Mousterian type, except in two minor differences, which were caused by the form of the raw material which the ancient flakers had at their disposal. I agree with Mr. Warren that his communication deals only with generalities, but I would add that these are of a completely erroneous nature and afford a somewhat dubious introduction to the “ further practical evidences ” with which we are to be favoured at a later date.

In the meanwhile a fully illustrated and detailed memoir of the Sligo discovery is being published by Mr. Reid Moir and myself (this has already reached final proof stage) which will enable those interested to form their own opinion as to the nature and significance of the Irish material. The geological objections to the age of the Sligo implements raised by Professor Macalister and his colleagues, and upon which Mr. Warren bases his case for rejecting these specimens, will be completely refuted in a few months' time, along with any other points that may, in the meanwhile, arise. I would, however, take this opportunity to supply the correct interpretation of the statement made by Professor Macalister and his colleagues with regard to the Rosses Point site, to the effect that “ Many of these fragments are so recently “ broken that they have not yet been rolled by the waves . . . . ” When I first discovered the collapsed rock-shelter at Rosses Point, the rock-platform outside the area occupied by the fallen roof-blocks was entirely free of fragments of rock, neither was a single piece of freshly-fractured rock discovered under the shelter. The material to which the Professor refers, and to which he attaches so

much importance, was due to the fracturing caused in the removal of the huge roof-blocks with the aid of crowbars and to the pitching out on to the rock-platform the smaller *débris* thus exposed. Furthermore, I carried out a prolonged series of experiments in fortuitous flaking. That is to say, I hurled rocks about, of varying size, after the manner in which some people imagine the sea does. The resulting fragments were left upon the rock-platform.

I suggest that Professor Macalister and his colleagues, together with my friend Mr. Warren, make a study of the freshly-fractured spalls discovered on the rock-platform, after which a comparison with the flakes and implements found by myself beneath the fallen roof-blocks would, I venture to think, prove of considerable benefit to them if they desire to arrive at the correct interpretation of the Sligo material.

J. P. T. BURCHELL.

### Siwa: Ethnology.

Cline.

**Notes on the Origin of the People of Siwa and Gara** By Walter Cline.

17

As the result of three months of field work last winter, for the Peabody Museum and the Harvard African Studies, I have some material which may throw light on the origin of the people of Siwa and Gara, in the Libyan Desert. Publication of this material from time to time may attract the co-operation of Berber scholars, and of ethnographers who have worked among the Tuareg, Tebu, and other North African groups.

The idea that the people of Siwa and Gara had a Tuareg origin is a very old one. It was based almost entirely on a few Siwan words which were collected by early travellers in this region, and which fitted rather closely the Tuareg vocabularies. Further study weakened this evidence. Mr. Oric Bates suggested, however, that the break in pottery technique since Roman times in Siwa, and the stitch-like designs on modern Siwan pots, indicate that Siwa was recently peopled by leather-working nomads.\* In addition, the following clues may be welcome.

Except the small outpost of Siwan culture at Gara, the Siwans know of only one other group who speak the Siwan language. These are the people of Aujla, north of Jalu. Mr. Francis Rodd, in his "People of the Veil," says that the Tuareg claim to have held the Aujla territory of old, from whence they spread southward and westward by a forced migration.†

An instructive lead comes out when we study the social position of the Siwan town-crier. In days of yore this officer not only cried the news, but kept the town gate, had charge of the public fire from which anyone could light a torch, and acted as interpreter between Bedawin and Berbers. They say that he was the only dweller in the Oasis who knew Arabic. He received public pay in food, as he does to-day. His office descends to his son or to one "near him." If he has no qualified descendant, they must appoint a successor from Gara Oasis, three days' march to the east; and in the old days he could not marry a Siwan woman, but had to select his wife from the score or so of unmarried females at Gara.

Why was this man, of Gara origin, prohibited from marrying a Siwan? Ahmad Abu Bekr, a record-keeper of Siwa, told me that the Gara people have descended from the slaves of the Bragia Bedawin, who wandered away from Gara long ago, leaving their servile class to cultivate the groves.

And how does this town-crier fit into the Siwan clan-system? I heard of him first as a member of the Aweynat, traditionally the oldest Siwan clan, and

\* Oric Bates, "Harvard African Studies," Vol. II, p. 299 *et seq.*

† F. R. Rodd, "People of the Veil," 1926, p. 348.

the only clan whose forefather, Ibrahim Gatli Auran, was originally a Siwan and had a Berber name. But later I stumbled on the fact that, though he "writes his name" with the Aweynat, he is really a member of a clan called the Auraghen, of which he is the only representative in Siwa. One of the *fikis* told me that the Auraghen and all the Gara people immigrated from Fezzan in the West.

Who were the Auraghen? The name is apparently the masculine plural of the Siwan *auragh*, meaning green, and a close cognate of the Tuareg word for yellow. Mr. Rodd, without mentioning such a derivation, discusses the origin and dispersion of the Auraghen Tuareg.\* He quotes from Duveyrier that "according to tradition they originally came from the neighbourhood of Sokna (in the Fezzan). Before establishing themselves where they are now located, the tribe inhabited in succession the Fezzan, the country of Ghat, and the Ahawagh, a territory situate on the left bank of the Niger, east of Timbuctoo . . ." Mr. Rodd describes them as a tribe in the Azger group and in Southern Air. "But whereas," he says, "in the Azger country the Auraghen are a noble tribe, in the southland they are a servile tribe of the Aulimmiden."

Before I have developed the comparative material, and have analysed the series of measurements taken on living Siwans, it would be rash to draw any conclusions from such superficial hints. I make, however, the following suggestions.

So far, owing to lack of data, physical anthropology has played no part in this question. The leading argument against the Tuareg origin of the Siwans is the dissimilarity in language and material culture between the two groups. But certain facts of folklore and social structure point the opposite way: (1) the migration of the Tuareg from Aujla, an oasis whose people speak Siwan; (2) the migration of the Gara people, who are Siwan in culture and language, from Fezzan, the home of the Auraghen Tuareg; and (3) the persistence among these Gara people of the clan name Auraghen, the name of one of the most vagrant of Tuareg tribes. I further suggest that Auran, the only Berber name which we find among the forefathers of the Siwan clans, may be a cognate of Auraghen; and that the Auraghen of Gara, a small and poor oasis, came in a later migration than that which brought the "Auran," speaking the same language, to the more desirable gardens of Siwa.

The social position of the Siwan town-crier supports this history. He was first employed as the Berber-Arabic interpreter for the Siwans, because he belonged to a tribe which, if not in itself half Arab, had come more into contact with Arabs during its later, nomadic career. As an outsider, and one of a servile class, he could not marry a Siwan woman. He is now enrolled with the Aweynat, the only Siwan clan of nominally Berber origin, because his office began before the immigration of Arabs and foreigners into the oasis had built up the Siwan clan-system.

These Siwi-speaking tribes received some of their blood and culture from the People of the Veil. We are eager to know how, when, and how much. If we drew our ideas from folklore alone, we should believe that both Siwa and Gara were settled by an eastern branch of the Auraghen Tuareg.

WALTER CLINE.

## Burma: Archæology.

Evans.

**Stone Celts from Northern Burma.** By I. H. N. Evans.

I have to thank my friend Captain J. H. Green of the 3/20th Burma Rifles for putting at my disposal the material described in this short note. All the

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\* F. R. Rodd, "People of the Veil," 1926, p. 347 *et seq.*, p. 366 *et seq.*

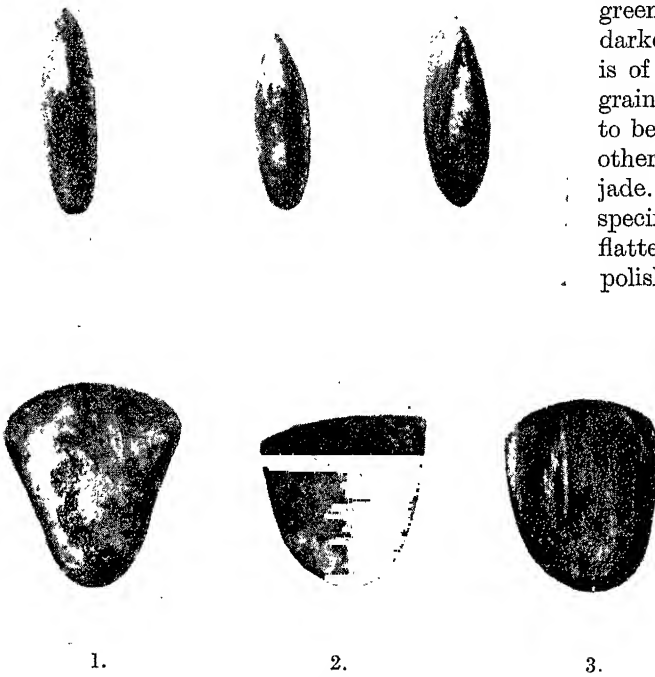


specimens were purchased by him from natives (Hkakhku Kachins) in the "Triangle," in Upper Burma, the country between the N'mai and the Mali rivers, which together form the Irrawaddy. This is a little-known region, but has recently been before the public eye owing to the slave emancipation operations conducted last year. Capt. Green's visit preceded this work.

Celts are called *Mu ningwa*, "Mu's axeheads," Mu being the *nat* of lightning. They are carried as charms by the Kachins and are occasionally found during cultivation of their mountain clearings. Capt. Green also saw larger specimens than those described and figured here—the size of the hand—which were of different shape. The natives refused to part with these. The implements obtained by him comprise eleven specimens, and the three illustrated are typical. The stone

from which, with one exception, all are made appears to be a light, greenish-grey jade with darker veins. The exception is of a dull, yellowish, close-grained stone, which seems to be softer than that of the others, but may also be jade. Only two of the specimens have been at all flattened at the sides by polishing. All of them have

been manufactured, without any chipping or flaking, by grinding down small water-worn pebbles of convenient shape, and, to prove this, in many cases, the coats of the pebbles remain towards their proximal ends,



STONE CELTS FROM NORTHERN BURMA.  
FACE AND SIDE VIEWS AT NATURAL SIZE.

while similar patches, which have not been entirely obliterated by grinding, are to be seen on their bodies and even towards their cutting edges. Such patches are to be noted on the face and at the proximal end of the implement on the left of the plate (No. 1) and on the proximal end of that on the right (No. 2). The cutting edges of the celts may be either somewhat bowed in outline, as in Nos. 1 and 3, or almost straight across (No. 2), while variants of the general shape are from somewhat fan-shaped (No. 1) to chisel-shaped (No. 2). Celt No. 1 is somewhat hollow hafted adze fashion. In No. 2, that portion which forms the cutting edge area has undergone intensive grinding and is, in consequence, marked off from the rest of the implement by a slight ridge.

I. H. N. EVANS.

Africa, East: Sociology.

Hatchell.

**Vibangwa—a Form of Insignia used in the Eastern Hinterland of Lake Tanganyika.** **19***By G. W. Hatchell.*

The area in which the *vibangwa*, to which these notes refer, are used is that bounded on the west by Lake Tanganyika from Karema to Cape Kungwe; on the north by a line running east from Kungwe; on the south by a line running east from Karema, and on the east by a line running northward from the point where the Rungwa river makes its bend southward into Lake Rukwa.

The *vibangwa* themselves are triangular shaped white ornaments, made from a marine shell and worn in the form of a fillet around the brows, a necklace, or singly around the upper arm or in the middle of the chest.

The people of the area are composed of a number of subtribes: immigrants from the Congo side of the lake or from the Malagarasi valley, who arrived some fourteen generations back and in some cases either conquered or peacefully absorbed the original inhabitants. Each division or subtribe is ruled by a chief and in the majority of cases has lost touch completely with its original Congolese or Malagarasi tribe. Many of these subtribes have become subdivided, for as the enthusiasm for governing diminished in the chief he frequently handed over portions of his area to his sons or nephews, who acted as his representatives therein. On his death these frequently became independent the one of the other.

When the British Administration, in 1924-5, decided on attempting to re-establish the old tribal organisation and the native administration it was faced with dozens of petty chiefs, each claiming to be independent and each claiming also to be paramount. In many cases these little chiefs had less than 100 able-bodied subjects; one man had but 25.

It became necessary therefore to search for something which would assist in deciding who was who. All the chiefs seemed to attach great importance to their *vibangwa* and it was hoped that an investigation of these would throw some light on the question.

It will be well at this stage to mention that all informants are agreed that the *vibangwa* were originally brought to the country from the coast by Arab traders.

The following are accounts of the usage in regard to *vibangwa* amongst some of the subtribes in the area in question:

**THE BAHOLOHOLO.**—These people are of Congolese origin. The *vibangwa* are in the custody of the Wapesi, who are the leaders in the ceremonies at the place where are deposited the skulls of the chiefs. A chief on succession is known as *mtwale* and on payment of from one to two large tusks of ivory to the Wapesi he may be invested with *vibangwa* and then becomes *mwami*. Investiture takes place at the place of skulls and all fires in the land are extinguished and relighted from a ceremonially-made fire at that place. The chief is conducted to the spot by the Wapesi, is introduced to the spirits of the dead chiefs and invested. It should be noted that this is the one and only time that the chief, be he *mtwale* or *mwami*, may go to the place of skulls. The Wapesi are the representatives of the reigning family of the original tribe in the Congo and in former times it was usual to send the Wapesi over for the ceremony, but as navigation became either more difficult or less convenient local representatives were appointed and a formal notification only was sent to the Congo side.

It was thought at first that possibly the *mtwale* having been invested became chief priest and king in one, but this was found not to be so, for a *mwami* cannot even visit the place of skulls, much less conduct devotions there. The skulls of all *mtwale* or *mwami* (a chief may never become *mwami* if too poor to pay the fees) are stored in a sacred and secret place at Karia, on the lake shore, and are tended by three old women specially appointed for the purpose; no trouble is taken over the body of the

chief but his skull is carefully preserved. *Mwami* Swima, in or about 1870, was killed in war in Upimbue, some hundred miles away at the head of the Rukwa Valley. His body was buried on the field, but his head was brought to Karia.

The Baholoholo state that they found no inhabitants in the country which they now occupy when they arrived there. Their present location is around Cape Kungwe.

THE WASHAMBA.—The Washamba live some 80 miles east of the Baholoholo and have their origin in the Malagarasi valley. The first chief was one Luhinda, who, as a result of family quarrels, set out in search of pastures new. He reached the country of Ushamba, which he found very sparsely populated, and gave to the heads of the people living there a girl in marriage and in return they allowed him to settle. As time went on he became more powerful and gradually absorbed the original inhabitants into his following, but he was still *mtwale*, for he had no *vibangwa*. He accordingly came to an agreement with the heads of the original inhabitants and was by them, for a consideration, invested with *vibangwa*. The descendants of these original heads are still the custodians of the *vibangwa* and it is at their hands that the Ushamba chief receives investiture.

THE WAKULUNGU AND WASENSE.—These people live on the Msaginya river some 80 miles east of Ushamba. Their origin was in one Schambwe, who came to that country on a hunting expedition. He found there a number of family units, who do not seem to have been organised under any chief, for it is related that so successful was he in providing them with meat that they asked him to be their chief. The area over which he assumed control was large and he appointed two representatives: one to control the north-eastern corner of his kingdom on the Ugalla river and one to control the south-western corner on the Usense river. The first of these, Matwiga by name, will receive mention later on.

There is no information that Schambwe was in possession of *vibangwa* when he arrived or that he obtained any from the representatives of the people he found in Ukulungu. On the contrary, it is stated by all authorities that he purchased his from an Arab trader, and invested himself with them after consultation with his advisers. He invested his Usense representative and Matwiga with *vibangwa*. Revolt broke out in Matwiga's country and, unable to quell it, he sent across the Ugalla or Shama river to the chief of the Wagalla for aid, promising him in return that portion of his area in dispute and a portion of his *vibangwa*. The chief of the Wagalla settled down, leaving a representative in his old area, to whom he in turn sent a portion of the *vibangwa* which he had received from Matwiga, who in his turn had received them from Schambwe.

About this time there arrived from the country of Mambwe, on what is now the Tanganyika-N. Rhodesian border, one Kamfwa, also in search of pastures new as a result of family quarrels. Kamfwa found the country at the head of the Rukwa Valley, some sixty miles south of Schambwe's country, pleasing to him, but before he settled down he applied to Schambwe for *vibangwa* and received them at his hands. Kamfwa's people were known as the Wapimbue, a powerful and warlike tribe who could have "eaten up" Schambwe with ease, yet Kamfwa applied to Schambwe for *vibangwa* before he settled down.

That portion of Schambwe's *vibangwa* which was sent to Usense became similarly subdivided. About the year 1880 serious family disputes broke out in Usense and by 1918 Usense had become subdivided into four petty chieftainates, each chief in possession of a portion of the original *vibangwa* given by Schambwe to Usense, and the position in Ukulungu was as follows: Mkomia, direct descendant of Schambwe, was in possession of what was left of the original *vibangwa*; Matwiga's descendants in possession of their portion and still owing allegiance to Mkomia; the Wagalla south of the Ugalla river still in possession of their portion but no

longer owing allegiance to either Matwiga or Mkomia; the Wagalla, north of the river, still in possession of their portion which they had received from their brethren south of the river but no longer owing allegiance to them but, on the contrary, endeavouring to assert their authority over them on the grounds that they are the descendants of the originals and that the *vibangwa* are of no consequence in that connection; the original Wasense still in possession of their *vibangwa* and still owing allegiance to Mkomia but the three subdivisions claiming complete independence; the Wapimbue still in possession of their *vibangwa* which they had received from Schambwe, but owing no allegiance to him or his descendants although they invite him to send his representatives to attend at investiture.

The foregoing examples will serve to show the customs relative to *vibangwa* in the area in question and it may be convenient here to note some of the religious beliefs of the peoples concerned.

All these peoples, whether of Malagarasi, Congolese or Northern Rhodesian origin, venerate the spirits of their ancestors. They believe vaguely in a supreme being and creator, and in four agencies, which, for want of a better term, may be described as spirits of the country, or soil. These in order of precedence live in Kungwe Mountain some forty miles south-east of Cape Kungwe; in Kabogo rock at the base of Cape Kungwe; in Lake Katabi, and in Lake Chada. The ancestral spirits act as intermediaries between the people and the "spirits of the country" (Kungwe, Kabogo, Katabi and Chada) and they in their turn act as intermediaries between the ancestral spirits and the supreme being but are invested with such great powers that there is some doubt whether they ever refer matters to the supreme being. The Baholoholo and other tribes of Congo origin venerate also and separately the spirits of departed twins, and these spirits seem to be considered more powerful than the ordinary ancestral spirits, with whom they do not live or associate.

Nothing in the course of enquiries into *vibangwa* came to light to suggest that the "spirits of the country" are concerned with *vibangwa*, and it was remarked that, although the people speak freely of Kungwe and his associates, they did not refer to them when discussing *vibangwa*.

Summarising the information contained in the foregoing we have:—

(a) *Vibangwa* are worked from marine shells brought to the area by Arab traders, shaped and ground to triangular form.

(b) The chief when an immigrant in an uninhabited country may be invested by his former paramount.

(c) The chief when an immigrant in an inhabited country may be invested by the representatives of those inhabitants.

(d) The chief may invest himself after consultation with his elders.

(e) He may invest his subchiefs with portions of his own *vibangwa*.

(f) He may invest a friendly and more powerful neighbour with a portion of his own *vibangwa*.

(g) There is nothing to show that his judicial and executive powers are enhanced by the possession of *vibangwa*.

(h) There is nothing to show that he becomes a religious officer on obtaining *vibangwa*.

(i) *Vibangwa* are not conferred free of charge; a payment of ivory or other currency is necessary.

(j) There is nothing to show that there is any connection between *vibangwa* and the "spirits of the country."

(k) There is evidence to suggest that there is a connection between them and the ancestral spirits.

(l) The chief on accession to the throne is not automatically invested. He may go through life without being invested.

(m) The custodians of the *vibangwa* are not members of the Royal Family.

In regard to (h) above: among the Baholoholo it is stated that the *mwami*, not the *mwale*, may, when a lion is killed, make public prayers for good crops to the ancestral spirits, standing in the middle of the village with his left foot on the head of the dead lion.

The *vibangwa* of a chief usually takes the form of a chaplet of the shells mounted on a piece of lion skin, but the wearing of *vibangwa* is not confined to chiefs or subchiefs. One frequently sees a commoner with one on a necklace, and among the people of the Rungwa river country similar shells are worn by the bride at her marriage. The people of Rungwa are of Northern Rhodesian origin and their chief does not wear *vibangwa*, although his people venerate Kungwe and his associates and a local female spirit who lives in a palm tree who is known as Mama Mwasi. It is related of her that she had the power of enriching the Chief of the Wa-rungwa by bringing elephants to die at the foot of her tree. The regalia of the Wa-rungwa chiefs consists, among other things, of a copper spear and drums. The latter are said to sound unbeaten by any human hand as a sign of danger and at the death of the chief, when all male animals, cocks, bulls, he-goats, etc., are killed.

G. W. HATCHELL.

## REVIEWS.

Japan: Religion. Katō: Hoshino.

*Kogoshūi: Gleanings from Ancient Stories*. Translated with an Intro-  
duction and Notes by Genchi Katō and Hikoshirō Hoshino. 3rd and enlarged edition. Tokyo: Meiji Japan Society, 1926.

*A Study of Shintō, The Religion of the Japanese Nation*. By Genchi Katō. Tokyo: Meiji Japan Society, 1926.

The Meiji Japan Society is symptomatic of the great change that has come over Japanese thought in the last score of years, for its aim is to study Japanese civilization, including Japanese religious ideas and Japanese historical traditions, "in the light of modern research and thereby to understand Japan and bring Japanese and foreigners to a better mutual understanding." When the present reviewer lived in Japan—in the "Meiji" era—neither of these books could well have been published by professors at Japanese universities, for both of them submit the very basis of Japanese state religion and Japanese myths and historical traditions about the origin of the Japanese reigning family to scientific criticism.

The *Kogoshūi* was a memorial, written by Imbe-no-Hironari in 807 A.D. and presented to the reigning Mikadō, Daidō, setting forth the genesis and national traditions of Japan and the Japanese royal family as preserved in his own family, and protesting against the reduction of the Imbe family to a lower court rank than that of the rival Nakatomi family, seeing that these early traditions showed that the divine ancestors of both families equally performed a part of the

ceremonies that induced the Sun Goddess, Amaterasu, to come out of her Heavenly-Rock-Cave and once more illuminate Heaven and Earth.

As might be expected, Hironari's Memorial is a mine of information respecting ancient Japanese traditional beliefs and rites as they were preserved in the family of a 9th century hereditary priest of Shintō, and Professors Katō and Hoshino deserve the gratitude of all English-speaking anthropologists for this annotated English translation with its full bibliography and its facsimiles of samples of the oldest extant Japanese MS. copies. (K. A. Florenz's German translation appeared in 1919). The text, being the deliberate composition of a single author, though covering largely the same ground, is easier reading than the 8th century *Kojiki* (Chamberlain) or *Nihongi* (Aston) (English translations); yet it irresistably suggests (to one reader, at all events) an oral tradition based on an earlier written version in which some clever editor has sought to combine and synthesise two entirely different sets of sources. Is it possible that the curious Izanagi and Izanami creation story (dismissed in a sentence here); the story of Susano-O's wanton provocation of his divine sister the Sun Goddess by endeavouring to injure her crops and defile her sacred hall, with its sequel in her retirement in high dudgeon to the Rock-Cave, the rites and dance performed to induce her to come out and put an end to the Darkness, and his own subsequent banishment, as a punishment,

to the Underworld (while hair from his head and nail-parings from his toes were cut off—to keep as magical hostages for his future good behaviour?), with the story of his Journey, beginning with a combat at a River with a monstrous Serpent from whose tail he wins a miraculous Sword which enables him to overcome all the dangers that beset him in future episodes; and then the further story of the Imperial Grandson's Descent to "The Luxuriant Land of Reed Plains" with his talismans the Divine Mirror, the (same) Magical Sword, and the Curved (stone) Jewels, together with the incidents of his Journey to the Wondrous Peak, and subsequent hints of ritual sacrifices of a White Horse, a White Boar, and White Cocks—is it possible that all this embodies a dim recollection of some solemn ritual Mystery Drama going back to times before the Yamato trekked across the Continent from their home somewhere in the region of Persia or the Caucasus?\*

The other elements with which it is now made to consort are so incompatible—phallic gods and their aniconic representations, and phallic agricultural fertilization rites; scraps of tradition preserving memories of a time when people lived on the sea-shore and had to sweep crabs away from the new hut built for a Sea-god's daughter to retire into to accomplish the delivery of her child, and when there was ceremonial building of new temples in which the first tree had to be felled with a consecrated axe and the first sod turned with a consecrated mattock, and when there were sacred guilds of wood-workers, weavers, spear-makers, and talisman-makers; of migrations in search of soil suitable for the growth of special plants used in the manufacture of temple tributes, and, most suggestive of all, of a cult that necessitated the setting up of enclosures with sacred trees and sacred stone pillars.

\* Some such Mystery Drama as survives in the Chinese Hung Society ritual (see "The Hung Society," by J. S. M. Ward and W. G. Stirling), and in the Mandæan writings, according to Svend Aage Pallis ("Mandæan Studies" (Eng. Transl.)). There is nothing inherently improbable in all three having got the skeleton of this mystery drama from the same source, since Andersson has found evidences of Mesopotamian contacts with prehistoric China on the borders of Kansu and Mongolia, and since Pallis considers that the oldest elements in Mandæan religion come from Persia in pre-Babylonian times. Among these earliest elements are many purificatory ideas, by no means primitive in conception, which also exist in Shintō, and an echo of which is seen from the living waters of the three rivers in the Hung initiation ceremony.

Japanese archaeology has got to explain—or explain away!—vestiges of civilisations equally divergent in kind and in development. There are megalithic chambered mounds which might have come from Spain or the Channel Isles, associated with bronze and iron weapons, with an austere funerary style of pottery (the forms of some of which reproduce, almost exactly, funerary brasiers now being unearthed by Mr. Woolley from Mesopotamian graves of 3000 B.C.), and with plastic representations of an armed chivalry whose facial characteristics are Caucasian rather than Mongolian; there are stone circles and menhirs; there are isolated finds, turned up by the plough, of obvious copies, in stone, of bronze weapons and wooden and other articles of daily use, perforated for suspension and suggesting a votive origin (the *Kogoshūi* tells of the gods who descended to earth hanging votive offerings on sacred trees intended to propitiate the gods of Heaven), of plaques and clay images some of which seem to find their closest affinities in the lower levels of the so-called Neolithic temples of Malta, and others with figures of Babylonian provenance; there are shell-mounds along the shores in which the oldest, and, in artistic conception the most elaborate, pottery, while reminiscent of later Siberian culture, also suggests affinity of artistic inspiration with the very early painted pottery civilisation of the Continent; and there are the unrelated finds of those curious bronze bell-shaped objects (*dōtaku*) with their significant panels of "running water" (*ryusui-mon*), chevron, and spiral decoration, which call up no analogous prehistoric form except, perhaps, Sir Arthur Evans's votive clay "sheep-bells" found in Crete in a Middle Minoan I. horizon.

Physical anthropology in Japan has not yet gone very far in its researches, yet far enough to suggest, in its turn, a composite origin for the population—which, indeed, is strikingly obvious to anyone who goes about the country observing the people's facial characteristics.

All these matters cry out for elucidation, and, with Mohenjodaro sprung on us in the Indus valley, surely it is as likely that clues to the origin of civilisation will come from China and Japan as from—let us say Glozel or North Germany! The written languages of China and Japan are great handicaps to that successful exchange of anthropological news and views between the Far East and the West which is necessary if we are to make progress in the human branch of what the French have felicitously called the Science of Origins. Hence the Meiji Japan Society, with its English publications, is to be

heartily welcomed, and it is to be hoped that it will push its investigations into the origin of Japanese civilization beyond the limits of literary tradition and will attack local prehistory through the archaeological remains of human artefacts.

The bias of Professor Katō's "Study of "Shintō" is revealed in its sub-title—"The Religion of the Japanese Nation." It is, however, the harmless bias of a man who sincerely believes that to-day "the tenets of Christianity, Buddhism, and "Shintō, the three living religions of the world, bound by universal brotherhood "in religious Japan, stand fundamentally "on one and the same Truth or Principle" despite variation in form. This bias enables him to study the development of Shintō with sympathy and insight, though it prejudices him against giving due attention to one aspect of its earliest phase, and renders him rather an unsafe interpreter of the interesting facts about primitive Shintō which his researches have brought to light. For instance, he seems unable to divest himself of his modern propagandist's attitude of condemnation towards the crude phallicism that he finds in primitive Shintō (and which he owns is still alive in secluded parts of the country), whereas a more detached and scientific approach to the subject would not only enhance the value of his interpretative work but would in no way alienate sympathy from the form taken by modern Japanese philosophical-cum-religious belief which prefers a Neo-Shintō to a Buddhist or Christian allegiance. His bibliography shows him to have limited his comparative reading in European languages mainly to works dealing with Hebrew and with classical Greek and Roman religion, and this may have contributed to this attitude of mind. The book, however, will be invaluable to students of comparative religion as a revelation by a Neo-Shintōist, who is also a great Japanese scholar, firstly of the reactions of thinking Japanese people, throughout history, to Religion, and secondly to their efforts to import philosophical or symbolical meaning into legends and rites which offended by their indecency. His early chapters, too, are full of valuable references to old Japanese ritual texts, and to contemporary records of early beliefs and rites, illustrated by interesting comparisons with their present-day survivals and modifications, neither the one nor the other easy to come by for the Western student. Anybody who has talked with thoughtful Japanese people who went through the terrible experience of the Tokyo earthquake and fire of 1923, cannot fail to realise that, side by side with the critical spirit that has succeeded Japanese

Romanticism in the last couple of decades, there is also growing up a disposition to universalist religious ideas in succession to the imported atheism which educated Japan privately adopted in the early Restoration period while in public supporting the revival of State Shintō. Contemporary Japan (taking the term broadly) is rather like what is misleadingly called a "slow motion" picture on the cinematograph: in the rapidity with which it passes from one phase to another it enables us to watch the process of culture-development under the influence of foreign contacts (or culture-disintegration, according to whichever aspect of a single process of evolution we choose to emphasise) taking place before our eyes. One ventures to suggest that a study of this telescoped evolutionary process in an advanced neighbouring civilisation is at least as likely to throw light on the motivation and sequences of culture-evolution in the great civilizations of the prehistoric past, as are the more popular studies of the naïve customs and ideas of contemporary peoples in a primitive stage of development who have somehow managed to lag behind, culturally, while the rest of humanity has moved on. Such a study must be more profitable than, for instance, the rigid typological classification of flint implements, which diverts so much brain-power from more fruitful investigation. If the Meiji Japan Society can facilitate such comparative study both at home and in the West, it will earn the lasting gratitude of Anthropology.

V. C. C. COLLUM.

Ice Age.

Bayer.

*Der Mensch im Eiszeitalter.* I. u. II. Teil. By Josef Bayer. Leipzig 21 u. Wien: F. Deuticke. 1927. Pp. x. + 452, 1 coloured plate, 220 figures.

In this work Bayer works out with a considerable degree of synthesis his reflections on his own and other men's, and notably Penck's, observations on the phases of the Ice Age and the attendant phenomena. He is very critical of the famous Penck scheme, though he is still more critical of what has been called the Monoglacial theory in this country. His discussions suggest the need for reconsidering the identification of a separate glacial phase called by the name of Gunz, and points which he does not appear to raise rather strengthen the suggestion. It is well known that Boule could not find traces of a Gunz phase in Western and Northern France; Renngarten does not find them in the Caucasus; nor does the Filippi expedition find them in the Hindu Kush, etc. An agreement that what Penck called the Gunz phase was some intro-



ductory part of the Mindel phase would be a simplification without loss of facts. Bayer would have us think of an "Early Diluvial Ice Age," and it seems that he considers the Chillesford and Weyburn Crags related to this, so he would make the "Tertiary" period end before these, in the Norwich Crag.

It is more difficult to reach an agreement as to what followed the long and fairly warm interglacial period after the Mindel phase of the Ice Age. Most students are agreed that this was the great interglacial phase. Penck thought it was followed by a Riss phase separated from the later Würm ice-maximum by a marked interglacial period. Bayer doubts the existence of this second interglacial on any large scale and denies that the (Penck's) Mindel-Riss interglacial fauna and flora returned to Europe in any subsequent interglacial stage. Here Hötting and Dürnten are of critical importance and there has been a widespread view that the Hötting breccia, and probably the Dürnten beds, very generally admitted to indicate a rather warm climate, overlies the Riss moraine and underlie the Würm moraine of Penck. If it should transpire that we might read Mindel for Riss the conflict between Penck and Bayer on this point would be resolved in Bayer's favour, but it is too soon to say that this is likely; rather should we express the hope that an attempt may be made to revise the identification of the moraines at these critical spots.

Bayer appears to favour a long Ice Age after his long interglacial and to look upon it as subject to many variations. This would be a sort of combination of the Riss-Würm-Bühl phases of Penck, with the acknowledgment of a temporary mildening before the last of these advances, a mildening associated with Aurignacian culture in Europe.

When we come to the identification of the Northern moraines with the Alpine and to fit in phases of culture, we find Bayer's interpretation at variance in some points with that which has been usual. The "Mindel" or maximum ice-sheet, and the extension of the later ice-sheet in "Mousterian" times are matters of fairly wide agreement, but Bayer thinks the Daniglacial moraine corresponds to the cold period he identifies with the Solutrean culture epoch, while the Gotiglacial moraine for him corresponds with the cooling noticed by all observers in the Magdalenian culture epoch.

It will generally be admitted that all our attempts to place the Northern and Alpine moraines in our scheme are as yet very tentative, but the writer of this review must record his feeling that the Fenno-

scandian rather than the Gotiglacial moraine corresponds to the Magdalenian cooling.

Bayer thinks that we can contrast about three widespread "Schotterterrassen" belonging to the main phases of maximum ice-extension of what he calls the Younger Diluvial Ice Age (approximately Penck's Riss-Würm-Bühl) with local terraces of intermediate phases. He recognises that his scheme of the Ice Age disturbs the parallelisms established between the Déperet system of terraces and the Penck system of Ice Age phases, and his view is that the Sicilian terrace belongs to his Older Diluvial Ice Age, the Milazzian and Tyrrhenian to the great interglacial period, and the Monastirian complex of terraces to that period of relative mildness in his Younger Diluvial Ice Age which he identifies with the Aurignacian culture phase. It is stimulating to have a fresh view of this complex subject, which, like that of the paralleling of Northern and Alpine Ice Sheets, obviously needs further meditation and observation. He thinks of the Loess as accumulating both just before and just after each Ice-Maximum.

These are but a few notes on some of the many points raised by Bayer. His work is rich in references to localities and their literature. It is stimulating rather than convincing, but we have not yet reached a stage of knowledge at which steadfast conviction is possible.

H. J. F.

Africa, West: Art.

Waterlot.

*Les Bas-Reliefs des Bâtimens royaux d'Abomey (Dahomey).* By Em. G. Waterlot. Paris: Institut d'Ethnologie. 1926. Pp. vi+10. 23 plates and commentary. Price

22

Ethnologists owe a debt of gratitude to the Institut d'Ethnologie of Paris for the series "Travaux et Mémoires," the first volume of which deals with the bas-reliefs of the royal buildings at Abomey (Dahomey). Historical documents of this nature are of so rare occurrence in Africa that their preservation and reproduction are of the highest importance, and their presentation in this volume leaves nothing to be desired. A short, but adequate, summary of the historical background precedes the illustrations, which are excellent, and, taken in conjunction with an explanatory commentary, enables the reader to correlate the latter with their contexts.

The series appears to have started about 1825, but fire and climatic conditions have played havoc with the mud-walled buildings, with the result that only the palaces of Glélé (1818) and Ghézô (1858) are intact (the former of which was restored in 1911), while the palace of Agadja (1708) is the



best preserved of the ruins. The bas-reliefs all relate to these three rulers, and it is noteworthy that they are only found on the walls of their private apartments, whereas the royal tombs contain no decoration. It may be remarked that here, again, the buildings were originally sanctified by human sacrifices, the living body of a King Dan being immured in the foundation.

The colours employed are, in the main, vegetable extracts; but ochre and kaolin are also used, fixed with palm oil and white of egg. It would appear, however, that colouring is comparatively late, as no trace of it has been found on the bas-reliefs of Agadja's palace, and the system of colouration became rapidly conventionalised and particular types were represented by their appropriate colours. Three polychromatic statues in wood, preserved in the Musée d'Ethnographie de Paris, representing Glèlé, Ghébanzin and Ghèzô, correspond with surprising fidelity to the allegorical bas-reliefs of the same kings which are reproduced here.

The interest of these bas-reliefs is twofold—historical and aesthetic. As might be expected from the circumstances of their execution, they very largely represent historical events more or less of a military character, incidents and dicta of the king of the period, often allegorically, as we see in the representation of Ghèzô as a buffalo—one of the most charming of the pictures in this *genre*—or of Glèlé as a lion. Here, no less than in contemporary metal work, we also note the importance of the royal baton, both historically and culturally.

In addition to these historical bas-reliefs, there is a good number of religious and semi-religious subjects, and it is at this point alone that M. Waterlot fails us, doubtless for the very good reason that the requisite information is not available. But we should like to know, for instance, the cultural significance of the bas-relief illustrated on Planche XVIa, and why an Offering to Ancestors should intervene in a series which is in the main historical: for that its appearance is not fortuitous we may be sure. Similarly, the very exquisite representation of the Rainbow Deity in the form of a serpent eating its own tail (Planche IXa) illustrates, as M. Delafosse points out, a frequent theme in African folklore, and its inclusion in the series of the palace of Ghèzô cannot be purposeless. A criticism may, perhaps, be permitted that the bas-reliefs as reproduced and displayed here have no integration: they are each of them individual and unrelated. Is it possible that there is an underlying unity of motive which might become apparent if they were studied as a whole rather than

as a collection of isolated pictures? A schematic diagram showing the positions of the bas-reliefs in relation to one another would have contributed to the value of a book which is otherwise a model of what such a book should be.

This is not the place for a detailed æsthetic criticism, but we cannot close this notice without drawing attention to the beauty of colouring and pattern, the exquisite lines and the simplicity of style which these bas-reliefs display. Judged as works of art alone, they attain a very high standard indeed. Planche XVIIIb is, possibly, the best thing in the book, perfect in design and colouring; and XVa is another supreme example of effective simplicity; nor could the shark-king of XXIIa be bettered in any way, so amazingly suggestive is it of power and speed with such an economy of technique. Of the earlier work from the palace of Agadja the conventionalised palm-tree of VIIa is, perhaps, the most effective. J. H. D.

#### Ethnology.

Murphy.

*Primitive Man, His Essential Quest*, by John Murphy, D.Litt., with a foreword by Dr. R. R. Marett, O.U.P. Price 15s. Pp. 341 + xi.

"The endeavour of man, both on the theoretical and the practical side of his life, is directed towards achieving a harmony within himself and at the same time in relation to his environment." The "essential quest" of man is the quest of unity. This is the main theme of Mr. Murphy's book. Evolution progresses by a series of alternating integrations and differentiations; each integration is more satisfactory than the last and as factors, such as an increase of population or the discovery of some important invention, arise to disturb it each gives place to fresh differentiations leading to a new integration. Taking this as his basis, the author traces the physical evolution of man and correlates with increasing co-ordinating power of the human brain, man's development, social, religious and moral.

At the outset he maintains that primitive man, in the strictest sense of the word "primitive," is comparable with the savage of to-day, and, in defence of this assumption, he points to the skull of the Australian aborigine with its low receding forehead and sets it beside that of Neanderthal man. It must be confessed however that certain customs and beliefs which he cites as typical of those with an undeveloped *neo-pallium* are not always drawn from people having this physical character. He suggests that there are, in the evolution of man, three main stages of integration: the

first is found in pre-human man, who is dominated by the integration of instinct; the second is when man is controlled by the integration of custom; the third when he has attained to the integration of intellect. The savage races are in the second, in "the bondage of custom." In primitive man "the brain lacks full integration and power of co-ordinating experience," and the savage has not therefore any ability for logical and sustained thought, for forming accurate or general concepts or for thinking in abstract terms. These deficiencies explain all the outstanding characters and beliefs in primitive society and religion; the difficulty of thinking logically for any length of time leads, through fatigue, to a complete subservience to the authority of custom or of an individual, and, through a desire to act rather than to think, to the performance of magical acts; while the want "of the power of forming concepts and the vagueness and wavering character of them when formed . . . accounts in great measure for the crude and strange shapes of man's early and savage religions," his beliefs concerning *mana*, magic, and *taboo*. Thus Mr. Murphy belongs definitely to the evolutionary school. Man's quest for unity, his strivings for integration, lead him to make inventions. The author does not, however, believe that the human mind is everywhere the same; it is "different at different stages of its development, just as the human brain is; and both are different in various races," and he disagrees profoundly with Levy-Brühl that there is a radical difference between the minds of primitive and civilised man.

Mr. Murphy's theory is interesting. It is unfortunate, however, that such facts as he produces to sustain it should be so often either inaccurate or taken at second-hand or from unscientific sources. His knowledge of ethnology is evidently superficial, as is attested by his sections on Totemism and Exogamy. What are we to make of the following passages? "Exogamy . . . marriage outside the kin, however kinship may be conceived, is not by any means universal in primitive life. It shares the field with Endogamy, marriage within the kin . . . Endogamy has . . . the appearance of being the product of reflection upon the sex relation in general . . ." "Exogamy . . . may be mentioned as the most striking example of semi-instinctive . . . reasoning, under the influence of a strong and very general repugnance to the marriage of persons closely akin." In analysing other aspects of savage life there is evidence of similar mental confusion. We are told that the Bushman is

slightly more advanced than the "lower West African negro" because the latter is fetichistic, while the former believe that the sun and moon are spirits and have life; and that the Hottentots, for similar reasons, mark a slightly higher stage than the Bantus. He evinces at times, too, a certain carelessness of thought, which results in what seem to be contradictions, and frequently in a confusingly loose use of words. Such phrases as "the instinct of kin," "an instinctive fear and taboo of dead things," are not uncommon, and he is fond of those overworked terms "*mana*" and "fetich."

His bibliography is very significant, for out of a list of sixty books, only six are descriptive works and of these only one is more recent than 1910. Had the author relied for his inspiration less on the works of Mr. Kipling and G. F. Watts and more on the results of recent scientific investigation, his book would have been more valuable. There is a danger in Anthropology of creating a fictitious "anthropological man" about whom to weave theories. It would be more satisfactory if those who write of the mind of primitive man would live for a time with these beings who have "generalised minds" and among whom "the customs are so implicitly obeyed that they might be called the instincts of the tribe."

CAMILLA H. WEDGWOOD.

America, North: Ethnology. Boas.

*Contributions to the Ethnology of the Kwakiutl.* By Franz Boas. 24  
Columbia University Contributions to Anthropology, edited by Franz Boas; vol. III. New York: Columbia University Press. 1925. Humphrey Milford, Oxford University Press. Pp. vi + 357. 24 x 16 cm. 21s.

Professor Boas has added to his long series of Kwakiutl texts a work of peculiar interest and of some novelty. His collaborator, Mr. George Hunt, a half-blood Indian who speaks Kwakiutl as his native language, and whom Professor Boas has trained in phonetic writing, has collected from his friends their recitals of sixty-eight dreams, and these are given in text with an English translation. The dreamers' names are given, and it would have been useful if the sex of the dreamer had also been marked; from internal evidence, however, seventeen of the dreams seem to be those of women. Obscene and ridiculous features are markedly absent, implying some reticence on the part of the narrators or the collector. Dreams of wish-fulfilment and frustration draw their incidents from hunting, fishing, food-gathering, love,

buying and breaking coppers, acquisition of occult powers, reunion with the dead, satisfaction of intellectual curiosity (No. 64). Other incidents are: pursuit by animals; sudden frights while seeking food; interviews with the dead (with the usual rebukes for coming to the place of the dead and warnings not to eat); visits to the sky; moral revelations; initiation to shamanism; revealed cures; interviews with spirits. Comparison of all this with the mythological themes of the North-west Coast is of obvious interest.

The second and larger part of the work forms a continuation of the series of texts

published in the 35th Annual Report of the Bureau of American Ethnology, "Ethnology of the Kwakiutl," and consists of fourteen original texts by Mr. Hunt, with Professor Boas's translations. These recount in full detail actual instances of the working of Kwakiutl social customs, especially the acquisition and inheritance of names. It would be difficult to exaggerate the interest of this circumstantial and concrete information; we should be the better for more like it from every part of the world. The book is produced by a photostat process.

BARBARA AITKEN.

## CORRESPONDENCE.

Anthropology: [Physical. Lundborg.  
**The Racial Characters of the Swedish Nation.**

To the Editor of MAN.

25

SIR,—In this review (MAN, 1928, 11), Dr. G. M. Morant writes, among other things: "The writers comment frequently on the fact that the data provided by previous workers are often found to be not comparable because the method of technique used was either unusual or undefined, *but they have omitted to give any adequate definitions of their own measurements. That strange oversight is a grave defect of this work.*"

The last statement, here italicised by myself, is entirely false, and only shows that the reviewer has not read the book very carefully.

The work in question, as a matter of fact, has a whole chapter devoted to "Collection

of the Material" (pp. 8-11). On p. 9, the card used for recording the measurements is shown (front and back) in English translation, and on this one finds the statement that "all measurements are taken according to Martin's 'Lehrbuch der Anthropologie,' 1st edition." The same statement is made on p. 8, the title, as well as the place and year of publication, being carefully recorded (Martin, R.: "Lehrbuch der Anthropologie," Jena, 1914). Here is thus a distinct reference to a generally known standard work of anthropology.

I am sorry that this false statement has been published in such a pointed form and in so respected a journal as MAN.

H. LUNDBORG,  
Director of the Swedish  
State Institute for Race  
Biology, Uppsala.

Uppsala, 9th January, 1928.

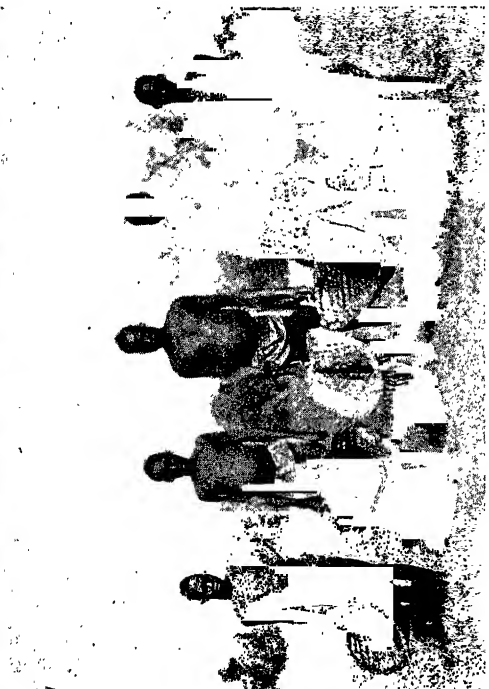
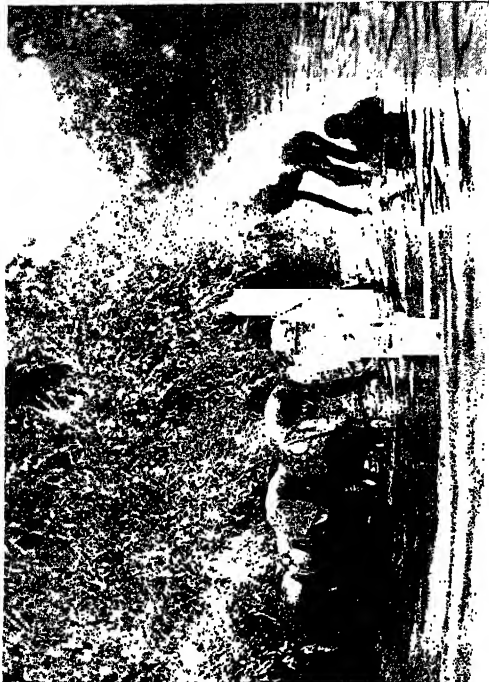
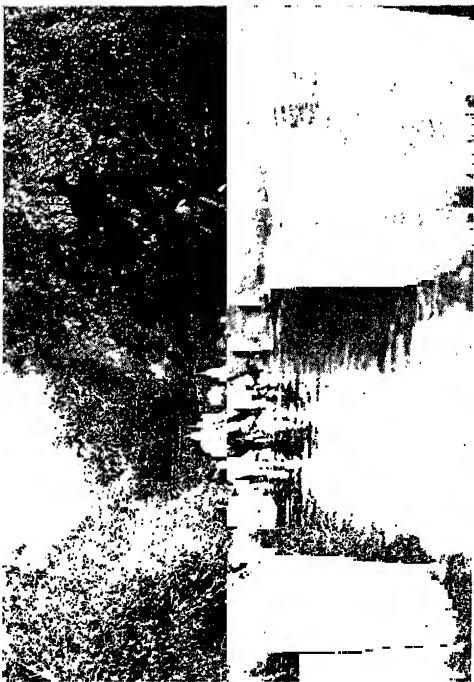
## ANTHROPOLOGICAL NOTE.

*Miss Caton-Thompson's Expedition to the Fayum.*—Miss Gardner and Miss Caton-Thompson have now been working for some weeks on the site in the Fayum for which a concession has been granted by the Egyptian Department of Antiquities to the Royal Anthropological Institute. Results of some interest have already been obtained, of which particulars will be given in a report to be issued later.

26

It is feared, however, that the expedition may be hampered by lack of funds, and the Institute appeals to its Fellows for subscriptions towards the cost. The total amount required is £1,000, towards which only a little over £300 has been raised. Dr. Rushton Parker has generously offered to contribute 10 per cent. on all amounts subscribed up to £1,000 before 30th June next.





AVOKAIYA FISHING.

## ORIGINAL ARTICLES.

With Plate C.

Sudan: Technology.

Evans-Pritchard.

**Avokaiya Fishing.** By *E. E. Evans-Pritchard.*

27

During a stay of six or seven weeks amongst the so-called Moro tribes on the west bank of the Nile, in Mongalla Province of the Anglo-Egyptian Sudan,\* I spent four days to the west of the Yei river, where the southern Moro are mingled with the Fajellu people and the Avokaiya, as this people is usually called. During these four days I twice went fishing with men and boys, not necessarily Avokaiya, but who used Avokaiya fish-scoops, in a watercourse where the flow had ceased and large pools remained. Few economic undertakings are more interesting to watch and to take part in than this dry-season pool fishing, and I give a brief description.

I need not describe the fish-scoops, as their form is clear from the accompanying photograph. They are made from *dura* (millet) stalks and are tied together at the openings with cord made from fibre. They are easily constructed in a few minutes.

Having chosen a likely pool the fishers submerge their scoops and, holding them at the sides with their hands, either draw them all together across the pool or push them in front of their bodies in a like manner. When the end of the pool is reached, the boys form a semicircle round the bank. As the scoops are being dragged or pushed by the older boys, the smaller boys splash in front with hands and feet and bodies to frighten the fish, so as to drive them into the scoops. The boys who form the ends of the line of fishers feel under the banks as they move along with a like object. If there are insufficient boys to form a line right across the stream, as is the case in the larger pools and as is shown in the photograph, then they cover what distance they can from one of the banks and small boys swim and splash on the open side of the pool.

Although, from the bank, the catching of fish by this means appears an easy task, in the water it is not found to be so. The fish, usually species of cat-fish, often jump over the scoops or slip through an opening between two scoops or between the end scoop and the bank. In these instances they are either seen or felt brushing against the feet and legs as they dart past. The boys give a shout and if the fish was a large one they will at once turn round and give pursuit. Some boys fish behind the line of scoops, moving independently to where they think a fish may be lurking. Small boys will sometimes obtain little fish by pushing rapidly a bundle of leaves with their hands against the bank, thus imprisoning the fish.

On the first occasion on which I was present one or two small cat-fish were caught after two to three hours' work. On the second occasion two big cat-fish and many smaller ones were caught after about four hours' fishing. This greater catch may be largely attributed to the greater organization of the undertaking. Some older men joined in the fishing and corrected the faults of the previous day. Their correction showed more clearly than could otherwise have been seen wherein lay the skill of using the scoops.

Firstly, these old hands tore open the closed tops of the scoops, showing that the mouths were not wide enough. The extension of the opening gave not only a larger catching-space but also allowed the sides of the scoops, held by the hands, to be moved more freely. Secondly, they showed how, when the fish has entered the scoop, the lip of the scoop should be turned up at once, thus closing the outlet.

\* My expedition to the Sudan was made on behalf of the Sudan Government and with additional financial assistance from the Royal Society and the Laura Spelman Rockefeller Trustees.

A large cat-fish had just escaped through lack of this precaution. Thirdly, they went along the line of boys making sure that the scoops were being drawn along the bottom of the pool and also that they were being pressed so close to each other that there was no opening under or at the sides of the scoops through which a fish could dart—for it is in this more than in any other part of the technique that success lies. Fourthly, they organized the small boys and taught them how to swim and splash about in the water so that their activities worked in co-operation with the manipulation of the scoops. They clouted any small boy who was trying to catch fish on his own with a bundle of leaves, and any other boys who were not acting in co-operation with the main activities. They also instructed two or three youths how to fish with their scoops in front of the main line, working into the holes of the bank which it would be difficult for the boys at the ends of the line to do without breaking the line and allowing the fish to escape.

When a fish is caught, the boy who is using the scoop and to whom, so I was informed, the fish belongs, runs with it to the bank and tips it out, and the small boys who are basking on the warm sands soon beat it to death with sticks.

Amongst the boys the fishing is more a play-activity than a serious economic undertaking. The fishing is accompanied by continuous shouting, chaff and laughter.

It was difficult for me to tell to what extent leadership in the activity was taken by the older boys, before the arrival of the men, as I was ignorant of the language. From observation alone I would say that the older boys certainly shouted instructions to each other and to the younger boys, but that this leadership was not invested in any one boy nor was its nature pronounced.

As the Avokaiya and the Moro are agricultural peoples and possess few cattle, fish are no doubt a welcome addition to the food supply; but I should imagine, from the size of the fish, roughly from 6 inches to 18 inches, which I saw, and from the few fish in the pools, that fishing in this manner does not play an important part in their economic life. But I had no time to enter into an investigation. The Moro Meza use fish poisons and the Moro Kodo also use nets and large hand-worked hooks. The Avokaiya probably have other means of catching fish than the scoop method described which are of greater economic utility and importance.

E. E. EVANS-PRITCHARD.

## Solomon Islands: Technology.

Wheeler.

### On some Pottery from Alu, Bougainville Strait, Solomon Islands.

By G. C. Wheeler.

28

*Kore* is the generic name for clay pots. The small round kind is *kulu* (Fig. 1, 2); the oval-shaped one is *fakasi* (Fig. 1, 1). The *kulu* and the *fakasi* have sides which belly out. *Fakasi* is also the name for the outrigger canoe (this, I was told, explained the name of this *kore*). *Angietina* is the name of a big *kore* with sides running straight up, and at the other end meeting in a point.

The clay (*kore*) for making *kore* is gathered by women; in Alu it is found at two places (near Paramata and Faleta); in Fauru at one place; and it is found in Mono (at Monosae (?) among others),\* where also *kore* are made. The clay so gathered (moistened if needful) is hand-rolled into small balls (*kubu*); and from these the making of a *kore* starts.† A big ball is made from two or three *kubu*, then beaten flat, and then beaten into cup shape against a stone (*patu*) held in the left hand. From some of the *kubu*, strips (*baku*) are beaten out. Around this cup one of the

\* Guppy ("The Solomon Islands: their Geology . . .," p. 98) mentions the "rich red ochre-clay" in Mono, some kinds of which are used for pottery.

† I saw part of the process of making a *kulu* by Tampa, the wife of Melo, the chief of Faleta.

*baku* is wound and worked in with it, making it higher and bigger; then another strip is put on in the same way, and so on, until the wished-for height and size of the *kore* is reached. The *kore* is now in a rough shape and moist, resting on a thick mat (*piuala*); it is now beaten with a flat stick (*kevava*), the left hand holding a stone inside against the part which is being beaten. A pattern is then printed with



FIG. 1.—1. FAKASI. Dimensions, 10.6" × 7.5"; height, 5.3." 2. KULU. Diameter, 7"; height, 4.9". Both pots made and collected in Alu, 1908.

another flat stick, called a *fose*, which latter also gives the finished shape to the pot. The rough edges at the top are cut off with a piece of fibre (*pundosi*) taken from the fibrous pieces (*nasi*) near the leaves of the coconut-palm. Thin rolls of wet clay are made, and stuck on round the top, and in pieces hanging down from it along the outside, making a design somewhat like wide inverted V's all round the top\* (*fapipili*, the strip round the top; *tonutonu*, the V-design below it; *goro*, the action of rolling out the strips with the palm of the hand to make the *fapipili* and the *tonutonu*). A pattern is put on the *fapipili* and *tonutonu* rolls by lightly pressing the top of a *mauai* shell against them; this pattern (small holes, etc.) is called *ogiogi*. The pot (*kore*) at this stage is left for a time. About six weeks later it is baked as follows: a loose blazing fire is made in the open, and the *kore* is put in it and left till the fire has burned down (in about half an hour as I saw it). Then it is taken out and

FIG. 2.—TOTONASIA, MADE IN BUIM, COLLECTED IN ALU, 1908-9. Outside measurements: diameter, 12.5"; height, 8.8."

\* Mr. Braunholtz suggests that the *tonutonu* is an imitation in pottery of the network sling in which handleless vessels were often carried in Melanesia (e.g., Fiji, New Caledonia).



left to cool. Presently it is rubbed with banana (skinned, I think) inside and outside, which ends the whole process. Next day it is ready for use.\*

There is one kind of *kore* called *totonasia* (Fig. 2). It is made up of a shallow circular bowl (*kore*) standing on a cylindrical pedestal (*totonasia*). The rim of the bowl in my specimen is notched. The specimen I bought (from Tampa, a woman of Faleta) was originally bought at Aku in Buim, and made by an Aku woman. It is used for boiling. This shape is also made in Alu (by women), but the art of it seems not to be known by many: Tampa could make it.

The three pots shown on the plate are now in the British Museum.

G. C. WHEELER.

## Africa, West: Anthropometry.

Talbot.

**A Note on West African Anthropometry.** By P. Amaury Talbot.

29

Detailed anthropometrical and other somatological observations have been made by me on some 7,500 West Africans, nearly all male negroes. There seems no possibility of my finding time to deal with these for several years to come, and it therefore appeared best to publish, without further delay, the little that has been worked out—viz., the cephalic index of the Sudanese and Semi-Bantu inhabitants of the Southern Provinces of Nigeria.

Information about these tribes will be found in "The Peoples of Southern Nigeria," published by the Oxford University Press last year; with regard to their numbers, habitat and languages in vol. IV. The provisional classification adopted therein, as stated in the preface to vol. I, had to depend almost entirely on the basis of language. The population is negro and was divided into Sudanese, Semi-Bantu and Bantu: the first term simply denoting those who could not be embraced under the last two.

The friend who worked out the figures gave the same weight to all the subtribes and clans in a tribe irrespective of their numbers.

To save space, only the tribal figures are printed, but I shall be happy to supply those for the subtribes and clans to any inquirer. It is regretted that the particulars for the Bantu-speaking peoples—who only amount to a small proportion, some 90,000—are not yet ready.

Group.	Tribe.	Number of Males.	Cephalic Index.
Sudanese -	Bali - - - - -	30	79.9
	Edo (including Bini) - - - - -	573	75.7
	Ibibio (including Efik) - - - - -	579	75.2
	Ibo - - - - -	2,603	76.4
	Ijaw (Kalabari) - - - - -	96	77.8
	„ (other) - - - - -	43	74.5
	Yoruba - - - - -	858	75.7
Semi-Bantu -	Bafumbum-Bansaw - - - - -	146	77.4
	Boki - - - - -	74	77.4
	Ekoi - - - - -	449	75.8
	Ekuri-Akunakuna - - - - -	79	76.3
	Iyala - - - - -	104	77.8
	Mbembe - - - - -	52	76.0
	Ododop - - - - -	19	74.5
	Orri - - - - -	16	76.3
	Ukelle - - - - -	23	75.9
	Uyanga - - - - -	20	75.0

P. AMAURY TALBOT.

\* So far as I saw the process of making a *kore* it was a *kulu*; the details as to ornamentation, of course, might vary in different cases. At the baking three *kore* were put on the fire together.

Sardinia: Art.

Suffern.

**Primitive Sardinian Art.** *By C. Suffern.***30**

In the spring of 1924 I was travelling in Sardinia and I noticed a type of carving and of embroidery which appeared to me to be peculiar to the island. I first saw it in the fine collection in the Pinacotheca of the Museo Nazionale at Cagliari. I found it repeatedly in various country districts throughout the island, notably in a small collection at Lanusei. I determined to buy a rug, if possible, and so on my last visit to Cagliari I tried to find examples in the shops. There were a few; but they were all so obviously made for the purpose of being sold as curios at high prices that they did not appeal to me so much as the genuine article made for actual use. Luckily, however, on my last day some peasant women came to the hotel to sell various bits of peasant work, such as rugs, corsages and kerchiefs beautifully embroidered, and I selected a rug which was alleged to be over a hundred years old and had certainly had a lot of wear.

This rug measures 80 inches by 26 inches and is a piece of coarse canvas bound round the edges with some green woven material. There is a dark blue rosette half way along each end, as though for the purpose of hanging the rug on a wall. The central part of

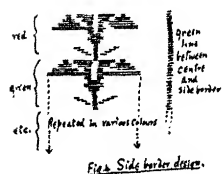
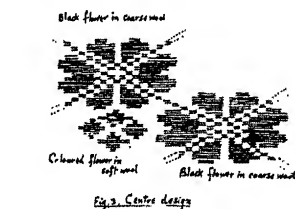


Fig. 4. Side border design.

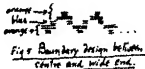


Fig. 5. Boundary design. In centre, outer and inner ends.

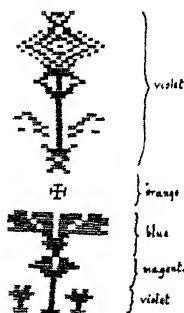


Fig. 6. Secondary and design. Repeating groups of three and four.

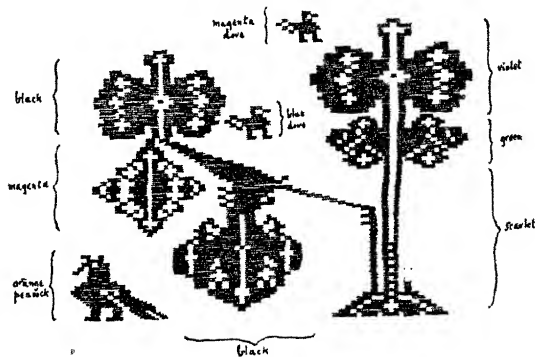


Fig. 1. Main and design. Tree with other. (The design is repeated, symmetrical when completed. That figure is incomplete.)

the rug measures 68 inches by 24 inches and is occupied by a floral design of black "roses" in diagonal rows worked in very coarse wool. In the spaces thus left are worked in soft wool smaller cruciform flowers in various brilliant colours (v. Fig. 3). Down the long sides of the rug are borders only 1 inch wide, bearing flowers, each flower being worked in one colour; but no two successive flowers are of the same colour, although the two opposite borders match each other in the order of colours used. These

borders are marked off from the centre by a narrow green line (v. Fig. 4), while a zig-zag line of blue and orange (v. Fig. 5) separates the centre from a wide border at each end of the rug. These latter borders are 6 inches wide and it is to them that I would particularly draw attention. The highly conventional design of a wonderful tree accompanied by birds is shown in Fig. 1. Each such group is separated from its neighbours by a design of two peculiar floral subjects (v. Fig. 2). I have omitted from the drawings a zig-zag black line representing the earth, upon which stand the trees and the larger birds.

This design is a typical Sardinian design; but, unfortunately, it does not contain the commonest object of Sardinian embroidery and carving—a peculiar and highly conventional fore-half of a stag. That this theriomorphic design is very ancient is shown by the conventional form and by the fact that doves and stags are prominently represented in the bronze votive statuettes of the Sardinian Bronze Age. I believe the smaller birds on the rug are doves, equivalent to the doves of Hathor in Egypt, Rhea in Crete, Aphrodite in Greece, and Venus in Italy. The larger birds are more like peacocks, comparable to the peacocks of Juno.

Although recent art in Sardinia seems to confine its attentions mainly to doves and half-stags, the Bronze Age statuettes have a wider zoological range, including, additionally, bulls and cows, cocks, wild boars, *mouflons*, sheep, goats and even apes or monkeys, the last being presumably imported or imitated from an import. I think that the reason why none of these additional animals has persisted into recent art in Sardinia is that wild boars, *mouflon*, goats and apes are merely due to a development of the naturalistic trend of Bronze Age art and, not being intimately connected with religious myth, have no reason for persisting, whereas doves, cocks, stags, bulls and cows are intimately connected with pagan religion. Of the latter animals, doves and cocks have persisted because they were assimilated as symbols by the Christian religion, which, on the other hand, has almost no room for symbols connected with bull-worship. Perhaps stags have also persisted (apart from ecclesiastical art, in which I have not noticed it) on account of their very close association with doves as pets or types of the primitive female goddess. Statuettes of a nude female deity have been found in Sardinia, likewise a Bronze Age "madonna" and "bambino" (v. Fig. 25 in Taramelli's "Guida del Museo Nazionale di Cagliari.")

The sheep or lamb has also been assimilated by Christianity and consequently does not appear in specimens of profane art.

Signs of bull-worship are very prominent in the relics of the Sardinian Bronze Age and perhaps the necessity of representing bulls and cows in that worship and stags and doves in the worship of the goddess led the artist further on in naturalistic art to the representation of the other animals already mentioned. These two worships were apparently combined in Sardinia. There is a statuette of a bull with a dove perched between his horns, parallel to another statuette of a bull with a cock perched similarly. There is also a remarkable collection of a large number of statuettes of hermaphrodites, which seem to point to a combined worship of male and female deities. This combination may perhaps persist in a much disguised form in the sacred tree and the doves of the rug.

From Santa Cristina and Paulilatino come two Bronze-Age statuettes representing votive ships or arks bearing doves along the sides, while one from Mandas represents an ark bearing a sacred tree or pillar surmounted by a dove. This is not the place to discuss the meaning of the ark in mythology, except to point out that it is a female symbol and is closely associated with doves (*cf.* dove of Noah, dove of Argonauts, etc.). There is also a model of a Bronze Age temple surmounted by a dove.

Stags occupy an equally prominent position in Bronze Age art in Sardinia. Examples include a votive sword surmounted by a stag, statuettes of stags, and (most important of all) a votive sword surmounted by a statuette of a warrior standing upon the concavity of a crescent (like the dove and the cock between the bull's horns) formed by two half-stags joined back to back (v. Fig. 35 in Taramelli's "Guida"). This last object belongs to a late time in the Bronze Age and accordingly the stags are very conventional but still easily recognisable. Here appears to be the Bronze Age precursor of the half-stag of recent Sardinian carving and embroidery. This treatment of the stag is parallel to the treatment

of the bull in the Sardinian Bronze Age. Thus the bull bearing a cock between his horns is only a half-bull on a votive ark, while other half-bulls adorn lamps shaped like arks. A similar half-bull is to be seen on a coin shown by Perrot and Chipiez ("Hist. of Art in Phrygia, Lydia, Caria and Lycia," p. 353), while a half-bull and a half-lion attached back to back (like the two Sardinian half-stags) are to be seen on a Lydian coin (Perrot and Chipiez, Fig. 189). The Bronze Age in Sardinia lasted from about 2000 B.C. to about 300 B.C. The earliest date of Lydian coinage is perhaps about 700 B.C.; but possibly the half-bull type of artistic representation is of much greater antiquity and may point to a connection between Sardinia and Lydia, which connection is a tradition and is supported by many other arguments. Sardinian bull-worship, the sacred double-axe, nuraghic architecture, plumed headgear, costumes and the peculiar type of naturalistic art point towards Minoan Crete. Stone statuettes found at Anghelu Ruju resemble those of Crete and other Ægean isles. Eneolithic vases from the same site have analogies with those of Crete. Copper slabs of the Bronze Age from Serra Ilizi bear letters of the Ægean alphabet. But the problem is not so simple. The nearest resemblances to the Sardinian statuettes do not come from Crete but from Cyprus and Rhodes, while others come from Etruria and Spain.

Crete is not the only land where theriomorphic artistic representations held sway and might possibly have influenced Sardinian art. Maltese examples can at once be dismissed as totally unlike anything Sardinian. There is, however, a faint resemblance to certain primitive Sicilian designs. For instance, the "peacock" of the rug shows a slight family likeness to one shown on p. 229 of Bisland's and Hoyt's "Seekers in Sicily." The following figure is a copy of it.



The chief points of resemblance are the crest, the prominent sternum, the thick legs and clumsy feet, the split tail and the body-markings. The cruciform arrangement of blank markings on the Sicilian peacock are to be compared with those on the Sardinian tree. (The cross is pre-Christian and only adopted by the Christian religion from paganism). Another design in the same book (p. 192) shows a man with an "extinguisher" or "mushroom" hat like that in Fig. 42 of Taramelli's "Guida." The Sicilian bears in his left hand what appears to be a model of a round hut, roofed with boughs, comparable to the modern Sardinian shepherds' booths, which are probably a relic of the Bronze Age.

I know of nothing in Egyptian or in Mesopotamian art resembling these figures. Thessalian figures and theriomorphic designs are quite different (v. Childe, *Man*, 1923, 2, and *Journ. R.A.I.*, 1923, pp. 263-288). On the other hand, affinities may be traced between the art of Sardinia and that of those parts of the western Balkans recently known as Bosnia, Serbia and Montenegro. Both areas are associated with prehistoric circular huts. Montenegrin embroidery patterns (Durham, *Man*, 1923, No. 40) show the rectangular pattern enclosing blank crosses and checkers as in the Sardinian sacred tree and representing in a very conventional form naturalistic objects such as fruit, flowers and a scorpion-fly. In Montenegro the stitch used is the primitive cross-stitch, the first stitch which is learnt in England by a child who is learning to work patterns for the first time, the stitch of the old English samplers. The Sardinians use a straight embroidery stitch, not a cross-stitch. The design on the rug, however, is not a curvilinear one but is rectangular, betraying its descent from a cross-stitch pattern. Even the curvilinear, i.e., later, Albanian designs include birds (? doves) and foliage on a bridal cloak and the return of the dove to the ark. In the same district have been found "little doves, pierced to wear as amulets" (v. Durham, *Man*, 1923, 40). It has also been suggested (v. Durham, *Man*, 1923, 21) that the name of Dardania is derived from *dardhe*,

meaning a pear. A list of pear place-names is given and the pear is said to be a traditional embroidery pattern. Figs. 2 and 4, illustrating the present article, may conceivably be interpreted as pears, the lower part of Fig. 2 representing the blossom and the upper part the fruit. One might also point to a suspicious resemblance between the words *dardhe* and *Sard*, which, however, I do not think are connected.

C. SUFFERN.

## Archæology.

Warner.

**The Problem of the Rosses Flakings.** *By S. Hazzledine Warren.*

31

During the earlier stages of my examination of the evidences, I said that I could publish nothing without visiting the site.

In certain respects, notably with regard to the sea as an explanation of the flakings, the evidences of the site are of primary significance, and in the beginning I made the mistake of being swayed by the reports of the circumstances of discovery which I had not been able to check for myself. Speaking within the limits of my own observation of the flakings, judged upon their own merits, I should place scapling first in probability; but I was, and am, open to accept the sea as the cause of the flakings if the evidences of the site indicate that to be so. On the other hand, I cannot consider Mousterian flake-implements, "choppers," "ovate hand-axes," "pointed hand-axes," and the like, upon the basis of anything that has yet been found.

The following is, as I see, the comparative evidences of the flakings:—

(1) *Scapling*.—From the first time I saw the Rosses flakings in July last, I felt that, as a group, they found their closest parallel, within the range of my own experience, in scapling-waste of the historical period. If the reader will refer to the descriptive diagram of the Rosses flakings given by my opponents in *Nature*, vol. 120, p. 443, he will see that they represent the technique of scapling, and of no previously known development of Mousterian work, whether in flint or other rock. I offer no opinion at the moment as to whether that diagram may be correct. I do not think that it is betraying confidence if I also say that others have more recently suggested that the Rosses flakings may be the scapling-waste from the making of stone anchors. For my own part, I should not specify stone anchors; there has been much desultory scapling, to shape stones for various purposes, all through history.

(2) *Marine*.—The breakers of the Atlantic, hurling stones against the low cliff, must of necessity make a great many flakes, and I have no knowledge of the special character that these flakes in the local rock may possess. Under favourable conditions, the sea produces very remarkable results.

(3) *Ice-pressure*.—It is quite likely that some derivative Boulder Clay flakings may be present on the beach of Rosses; without visiting the site one cannot say. But, as a whole (and one must always judge these things in groups), the distinctive characters of ice-pressure are conspicuously absent from the collection that I have seen.

(4) *Prehistoric Industry*.—I did not reject a prehistoric interpretation hastily, but for a considerable time held it equally in mind with its opposite. I had, and have, no prejudice or bias in the matter. After due consideration, I reached the opinion that the Rosses flakings gave no suggestion of prehistoric technique, or of any intention directed to the making of any form of useful implement, either palæolithic or neolithic.

I have not yet seen reason to modify that view.

It is quite likely that both scapling and marine flakes may be mingled together on the storm-beach of Rosses, but that only an examination of the site could

determine, and it is of secondary importance. It is the question of the prehistoric industry that is the main issue, and I should not have gone beyond the "non-prehistoric" without personal acquaintance with the evidences of the site.

To summarise the situation. In my judgment, the comparative study of the flakings gives no justification for inferring a prehistoric industry, while on the testimony of competent observers the Rosses situation is modern, recently eroded by the sea, and there is no justification for inferring a prehistoric site.

S. HAZZLEDINE WARREN.

## Archæology.

Burchell.

**An Analysis of Mr. Warren's Views on the Sligo Artifacts.** By

32

J. P. T. Burchell.

Protagonists are rarely able to settle their own differences and since Mr. Warren has written me privately expressing his disapproval of my recent article published in MAN, 1928, 16, I seek the earliest opportunity of making public the various phases of this controversy in the order in which they occurred. They are as follows:—

1. On, or about, the 28th of July, 1927, I received a visit from Mr. Warren, who made a critical examination of the Sligo material and expressed the opinion that the specimens represented either palæolithic waste or else quarrymen's refuse.

2. On the 28th of July Mr. Warren wrote me a letter in which he said that if the Sligo material had been found at Graig Lwyd it would have been quarrymen's refuse "in every way normal and characteristic." He continued by saying that if the case were his own (so far as he could judge without a knowledge of the site) he would hold the two alternative theories of prehistoric waste or quarrymen's refuse "about equally in mind."

3. On the 25th of September Mr. Warren wrote me a further letter in which he commented upon an article of mine published in *Nature* on the 24th of that month. He said that the chief impression he gathered from my series was its similarity to quarrymen's refuse, "on other points, on many other points to those you name in your letter."

4. On, or about, the 6th of October I received a further visit from Mr. Warren, who made another critical examination of the Sligo material, at the conclusion of which he expressed the opinion that the specimens represented quarrymen's refuse. At this meeting I pointed out that it was his bounden duty to restore all those people I had misled to the straight and narrow path of truth, and I suggested that Mr. Warren should write a letter to *Nature* therein expressing his views. Mr. Warren said he doubted if *Nature* would accept for publication a letter written by him, whereupon I replied by saying that I would do my utmost to secure publication of any letter he might choose to write. My offer was accepted as "generous."

5. On the 5th of November, a letter from Mr. Warren appeared in *Nature* in which he stated that he had had good opportunities for examining the Sligo material. No mention of prehistoric waste or quarrymen's refuse was made in this letter.

6. On the 1st of January, 1928, Mr. Warren made a claim for a natural origin of the Sligo specimens.

It only remains for those interested in the matter to arrive at the conclusion as to whether or not my article of February, 1928, accurately reflects the facts outlined above.

J. P. T. BURCHELL.

## REVIEWS.

Asia Minor: Archæology. De Genouillac.

*Céramique Cappadocienne, inventoriée et décrite avec une introduction* 33  
par Henri de Genouillac. Paris: Geuthner, 1926. Musée du Louvre—Département des Antiquités orientales. Série Archéologique I, II. Pp. 126 and 75. Pls. 21 and 57. Text figures.

The two volumes with which the Louvre opens a new series of publications reveal a surprising growth of the collection of Anatolian pottery in Paris. M. de Genouillac has produced an admirable catalogue, with beautiful collotype plates, sometimes coloured, and supplemented by line drawings when some detail required it. The first volume contains the well-known collection of Chantre, which has gone from the Musée Guimet to the Louvre, and of which a number of important pieces are now published for the first time. Those pictured sufficiently in Chantre's own book are not shown again here. The second volume, more than 200 numbers, contains new accessions to the Louvre. The descriptive catalogue is preceded by an introduction, which, as is usual in similar cases, is not very satisfactory because subjects requiring elaborate research have to be treated in a very limited space, if the character of a catalogue be preserved. This is most obvious in the sketchy chapters dealing with the ceramics of neighbouring regions and with chronology. The first topic contains the clue to the latter, as long as no systematic excavations are carried out in Cappadocia itself; but it is a vain attempt to deal with these problems in less than twenty pages. And thus the author has to admit that he is at a loss to define the sequence of the various fabrics and that his tentative suggestions are frankly guesses.

On the other hand, a few pages are devoted to a discussion of the wavy line, a motive which surely does not deserve this distinction. Here, as in Susa and elsewhere, its appearance is due to a general slackening of an originally rectilinear design. One may admit that certain primitive tribes, such as the Indians of the North Pacific coast, whose decorative art is studied by Boas, use very simple geometric designs as symbols. But it is unsafe to extend such an interpretation to the peoples of the ancient Near East, where there is no way to check the symbolistic "explanations" which we read into the poor pot-painter's scrawls, and where we have to deal with the inheritors of a purely decorative art of long standing: people, moreover, possessing script for the expression of their thoughts. To see

"water" in the wavy line or in the guilloche, and interpret spirals as "a stylised representation of the sea," or even as "clouds of dust," which therefore decorate ceilings or pavements in Crete (p. 40) may be an entertaining play of the imagination, but it seems not relevant in the scientific interpretation of ancient monuments.

Firmer ground is reached when the animal representations of the Hittite hieroglyphs and those modelled in clay are compared, and when finally a comparison is made between the actual pot-shapes in the collection and those which occur on the seals and on the seal impressions. This is a valuable methodological experiment which must have cost the author much painstaking labour; and the material here collected should continue to prove useful. The smallness of the seals and the consequent lack of precision of some designs, and the probability that a number of the vessels represented would not be of pottery, but of metal, imposes limitations. Certain it is, anyhow, that the "teapot" occurs already on the impressions of the "Cappadocian" tablets, a fact which we had presumed on technical grounds for the specimen found by Chantre, and which is confirmed since by Professor Hrozný's finds.\*

We shall best do justice to the important new material contained in this publication by discussing it in the cadre of the history of Anatolian ceramics which we recently tried to establish.† We have found that the earliest pottery of Asia Minor was black carboniferous ware which becomes red slip ware early in the Copper Age (Yortan, Pisidia), and the new technique naturally leads to the displacement of the utilitarian red slip by a merely decorative red wash.‡ This stage was reached well within the period of the existence of Hissarlik II; but though the three fabrics which succeeded each other could be traced all through the peninsula it was difficult to see to what extent the East, e.g., Cappadocia, formed a homogeneous province with the more western parts of Anatolia. Now M. de Genouillac publishes three vessels which prove this unity to have indeed existed. The oldest one is a primitive theriomorph vase from Cæsarea, with a handle on the back and the spout placed on the hind-quarters. It is closely

\* "Syria," 1927, 1 sqq. and Pl. IV, 5.

† H. Frankfort, "Studies in Early Pottery of the Near East," II, 57-63 and 153-179. Hereafter cited as "Studies."

‡ "Studies," 57-63.

related to the examples from Hissarlik and Yortan (II, Pl. 3, No. 72; *c.f.*, "Studies," II, 58, n. 2 d). The next vase, from Kültepe, is a beautiful beaker in red-wash ware with two large handles, of the type typical for the middle period of Hissarlik II and of Bos Euyuk (II, Pl. 49). Thirdly, there is a two-handled cup in the technique of the bowls with the polished red cross from Hissarlik V, which appear with the Hyksos period in Palestine, together with other red-wash wares from Anatolia\*. The newly-published specimen (II, Pl. 30, No. 111) provides a welcome example of the existence of this technique of decoration in between the two regions from which it was known hitherto.

The red-wash ware persists in Cappadocia into the second Hittite empire, about 1500 (II, Pl. 43, No. 170; 45, No. 108; Pl. 27, etc.). But in the last centuries of the third millennium there appears also painted ware there, simple sweeping designs, in free-field arrangement, based on the multiple zigzag. The technique is the same as one known at Anau I: matt black paint on a polished red ground. It is explicable as a product made where the Anatolian province of red-slip ware touched the Persian province of painted pottery. Thus its appearance at Susa, towards the end of the first civilisation, is well explicable.† The more so as stamp-seals not unlike those found with this pottery at Susa, and also in Anatolia, are now recorded from the Caucasus region.‡ Its westward extension into Cappadocia may well have fallen in the period contemporaneous with the "First Intermediate" in Egypt (about 2400-2200 B.C.), which, as we have shown elsewhere,§ was one of turmoil all through Western Asia. Of this earliest painted Anatolian pottery the new publication shows a few examples (I, Pl. 3, No. 9,766; Pl. 17, Nos. 9,819, 9,821, 9,822; II, Pl. 31, No. 121). Others are at Berlin||, and our chronology based on stylistic criteria is recently strikingly confirmed by the fact that Hrozný has found some jugs painted in this style with the "Cappadocian tablets" at Kültepe¶. The red-wash ware and this particular class of painted ware exist thus in Anatolia before the rise of the Hittite empire; and they also persist when the latter arises out of the turmoil of the opening centuries of the second millennium B.C. These two ceramic styles

correspond no doubt with the old aboriginal Asianic elements in the new state; but the numerous foreign elements which we know to have formed part of it as well are represented by strange features which penetrate into the decoration of the painted pottery and result in a hybridic truly Hittite style (I, Pl. 10; II, Pl. 10*bis*; Pl. 31, No. 126; Pl. 23, etc.). There are concentric circles which derive from North Syria as do also the trichromy, the metope-style and the animal drawings in silhouette—features which penetrate in this same period into Cyprus and into Egypt. Furthermore, we find in the Hittite pottery certain highly distinctive groupings of pot-hook spirals, known at an earlier date in Macedonia in exactly the same way, so that they recall Herodotus's and Strabo's statement as to the descent of the Phrygians from the Macedonian Bryges; and thus a Phrygian element seems also to have entered into the heterogeneous fabric of the Hittite empire. At present we can only assert this for the younger Hittite empire, the centre of which was Chatti (Boghaskeuy), founded apparently not much before Shubbiluliuma's reign; from that site and from Kara Eyuk most of our material derives. It may thus, on the whole, belong to about 1400-1200 B.C. But we do not know whether the blending of the various foreign features with the old Anatolian style of pot-painting was effected rather suddenly at the new capital of the extended realm, or whether it was a gradual change, occupying several centuries before the rise of Shubbiluliuma.

Now M. de Genouillac publishes very important evidence which seems to belong to the older Hittite empire. It was already mentioned but never published by Professor Curtius: certain vessels of red-wash ware show a Hittite design stamped in when the clay was still wet (II, Pl. 24, 25). And we are now given also the parallels from Hittite stamp-seals, which, as M. de Genouillac and M. Contenau have made probable, belong to the older Hittite empire. Whether this design is rightly called a "marque royale" I am unable to say, but the importance of these vessels is obvious.

A special feature of the collection of the Louvre is, further, its richness in clay plastic; and, with a few exceptions, all the material belongs to vases: it consists either of theriomorph vessels or of handles and spouts in the shape of parts of animals. The theriomorph vases contain most important material for the history of the *rhyton*. In the first place there are four specimens of the horn-shaped type, which ends below in the front part of an animal with an outlet in the breast. Von Bissing,

\* "Studies," 168.

† "Studies," I, 38.

‡ *Liverpool Annals*, XIV, 55.

§ *Journal of Egyptian Archaeology*, XII, 80 sqq.

|| "Studies," II, Pl. IX and Fig. 18.

¶ "Syria," 1927, Pl. IV, 2.



who has discussed this type\* quotes an exactly similar one found by Petrie in Egypt (XVIII Dyn.)† and insists on its long persistence by figuring a Persian one. Here belongs also the gold-and-silver rhyton recently obtained by Mr. Woolley for the British Museum‡, which he considers late Hittite and von Bissing old Achaemenid. Very remarkable are three vases in the Louvre (II, Pls. 5, 11, 12) showing kneeling animals which have an opening in the breast exactly like those of the horn-shaped rhyta with animal protome. Two of these vessels are called Ionian; but as far as one can see from the plate they are more Asiatic than Greek, and there seems to be no reason to separate them from the third, which is decidedly Hittite. They have all three a rimmed opening on the back, and this feature connects with the well-known Hittite vases in the shape of standing animals§ which, however, seem never to have an opening in the breast nor one in the mouth, differing herein from the silver stag of the IVth Shaftgrave at Mycenæ, which otherwise belongs to the Anatolian family. The three vases which present, to my knowledge, a new type, seem thus to be a mixture of the real rhyton with animal-protome and the ordinary theriomorph vase, which is, as we have already seen above, of old Anatolian standing. On the other hand a careful study of the numerous fragments of animals in clay may yet reveal that either or both of the types of rhyton go back to an earlier past. Here, however, one would have to handle the pottery itself. M. de Genouillac assumes a connection between all these vessels, the theriomorph vase representing a sacrificial animal, and the rhyton being an elaboration in which . . . . . the blood from the victim . . . . . by means of the opening in the breast. In that case the three vases mentioned above would form a transitional stage between the theriomorph vase and the rhyton. In addition to all this, another rhyton of a new type is pictured (II, Pl. 26) in the shape of an ordinary vase with two handles fixed on the shoulder by plastic bull's heads. The outlet is here in the base, and the shape is not paralleled by the quoted L.M. III funnels, but by certain Middle Minoan rhyta, e.g., from Zakro and the steatite Harvester-vase. Among the Ana-

tolian pottery it is somewhat exceptional, with its strong vertical burnish, typical at various times of Syrian slipped wares, and also because of its decoration, which consists of two horizontal white bands. But we certainly know too little of Cappadocian pottery yet to exclude this specimen from it. As other M.M. shapes are found in early Hittite pottery\*, we have to deal here with a problem which complicates that of the rhyta discussed above, and at present one can only recommend the important material contained in M. de Genouillac's volumes to the attention of investigators. This last vessel is of particular importance because its Minoan parallels belong to the *earliest* rhyta in Crete. That one should not be too hasty in assuming that Crete was again the country of origin is shown by a rhyton from Palaiakastro (B.S.A. X, 206), which shows a plastic ibex-head, a feature quite isolated in Crete, and, as we shall see, very common in Cappadocia. (See II, pp. 29 and 35.)†

More definite conclusions can, however, be reached on two other points, one affecting Homeric Troy and the other the situation of Keftiu. As to Homeric Troy, Hissarlik VI, its western connections, have been much too much stressed in the discussion of its civilisation. This was to some extent unavoidable, because no Anatolian material for comparison was known. Professor Hubert Schmidt, however, has noted already a Caucasian-Mesopotamian mace-axe and even the sheath of a Hittite curved sword amongst its remains.‡ These might have got to the Troad by trade, no doubt. But the popular product *par excellence*, the pottery, belongs at Troy to the Anatolian family by its shape. And now the exceptionally rich collection of theriomorph spouts and handles from Cappadocia in the Louvre

\* "Studies," Pl. IX, 1 and p. 177.

† See on the Cretan rhyta Karo's article in *Jahrbuch des Deutschen Archaeolog. Instituts*, 1911, 249 sqq. One might perhaps consider some bull-shaped vases from the Mesara as rhyta (Xanthondides, Pls. II, 4126, VII, 5052, XXXVII, 5053). The first two prove by their handle and the placing of the spout to be connected with Anatolia, which is also otherwise influencing the Early Minoan Period. The Mochlos example (Seager, p. 60) has eyes and mouth pierced like those from the Mesara. There is no survival of this type and next appear the late M.M. vases discussed in the text. The typical late Minoan rhyton consisting of an animal's head only is also paralleled in Cappadocia (e.g., II, Pl. 25). The use of rhyta may after all be one of the various features of religious ritual common to Asia Minor and Crete.

‡ In *Prähistorische Zeitschrift*, IV, 26, 28.

\* In *Archæologische Anzeiger*, 1923-24, 105 sqq.

† "Hykos and Israelite Cities," XXXVIA.

‡ In *Liverpool Annals*, X, Pl. LXVIII.

§ II, Pl. VIII, Pl. IX, No. 142; "Studies," Pl. XI; Ed. Meyer, "Reich und Kultur der Chetiter," Pl. V.

|| Maraghiannis, "Antiquités Crétoises," I, 32, 16.

shows how close the connection was; for every type known from Hissarlik VI can be paralleled by a Cappadocian specimen.\* The occurrence of Minyan and L.M. II were suggested already an early beginning for Troy. The new material suggests that it was so closely related with the Hittites that its *floruit* may well have coincided with the empire of Chatti, viz., 1600-1300 B.C.; and the traditional date for its fall (1281 B.C.) suggests that this event is part of the break-up of the Hittite empire.

The last point raised by the publication under discussion bears on the Keftiu problem. Amongst the theriomorph vase-handles in this catalogue goats and ibexes are conspicuous. (I, Pl. 5, Nos. 10001; 10002; II, Pls. 10, 20, 48.) They are not common in the Eastern Mediterranean or in Egypt†, but they occur with striking frequency in Theban tombs of the XVIII dynasty, amongst the tribute which Mr. Wainwright has sorted out as brought by the Keftiu people.‡ We cannot here enter into arguments from the geographic lists and from other indications which make it probable that the Keftiu were inhabiting the Cilician plain, and that their civilisation showed alongside with Asiatic also certain Aegean features. We may only mention the extraordinary importance of a new piece of evidence recently published by Dr. Valentin Müller, a silver pinax, actually found in a Cilician cave, and which shows a man of that Anatolian type wearing the Cretan metallic girdle.§ And now the occurrence of ibex-handles on the one hand in vases of the pictured Keftian tribute, and on the other in the Cappadocian collection of the Louvre, adds further weight to the argument.

We may end by stating that the descriptive text of the Catalogue is equal to the excellent plates in its accuracy and completeness. H. FRANKFORT.

\* Compare Genouillac I, Pl. 6, No. 10022 (horns broken off) with Schmidt, "Katalog Schliemann Sammlung," No. 3226:—Gen. Pls. 26; 19, No. 89; Pl. 53; with Kat. 3285.—Gen. Pl. 20, No. 65; Pl. 37, No. 88 with Kat. 3259.—Gen. Pl. 20, No. 101 with Kat. 3265.—Gen. Pl. 30, No. 71 with Kat. 3277.—Gen. Pl. 37, No. 86 with Kat. 3251 (compare perforation of latter with Gen. Pl. 16, No. 52.—And see also Gen. Pls. 17, 18, 53, 54, 55.

† See Mr. Glanville's remarks in *Journal of Egyptian Archaeology*, XIII, 53.

‡ In *Liverpool Annals*, VI, 24 sqq; and Pl. XIII, No. 89; and also 88 and 79.

§ In *Athen. Mittheilungen*, L, 63 sqq., with Fig. 6B and Pl. VI, 3. See now also his "Studien zur Kretisch-Mykenischen Kunst," II, 2 in *Jahrbuch d. Deutschen Archäologischen Instituts*, 1927.

## Australia:

### Ethnography.

Spencer & Gillen.

*The Arunta: A Study of a Stone Age People.* By Sir Baldwin Spencer and F. J. Gillen. London: Macmillan, 2 vols. 1927. Pp. i-xxviii, 1-646. Copiously illustrated. Price 36s.

Readers of MAN have no need to be assured of the great value of Sir Baldwin Spencer's new edition of "The Native Tribes of Central Australia" under the title of "The Arunta." The first edition in 1899 was immediately recognised as being a most important and authoritative work on the tribes of Central Australia; indeed, it was then the only monograph of any Australian tribe, and thenceforth the Arunta became more prominent and more discussed in anthropological literature than any other people. This epoch-making work was followed by others of scarcely less interest: "The Northern Tribes of Central Australia" (1904), "Across Australia" (1912), and "The Northern Tribes of the Northern Territory of Australia" (1914). Later investigations were made by Baldwin Spencer which enabled him to clear up points which had been raised in discussion and also to consider the statements made by the late C. Strehlow. It is evident that a great deal that Spencer and Gillen noted in 1896 has now entirely vanished owing to the deaths of old men who alone knew the old lore and even to the disappearance of whole groups. "There are but few of the older, unspoilt Arunta men left anywhere, and soon there will be none, and with them will pass away all knowledge of primitive customs and beliefs."

The descriptions in the first edition of ceremonies observed by the authors naturally remains practically identical in the new edition, but modifications have been made in various explanatory or comparative accounts of the ceremonies, and in a few cases there are alterations in the spelling of words, or even in the words themselves: thus the famous *Intichiuma* ceremonies are now called *Mbanbiuma* which is a widely recognised term throughout the whole tribe for those ceremonies in general, *Intichiuma* being merely one of the local terms; we have also to substitute the word *Alchera* for *Alcheringa* (*ringa* = "of" or "belonging to"). Like the term *churinga*, the word *Alchera* can be used both as a substantive and as a qualifying noun; it is not applied to any individual, and one of its fundamental meanings is "dream." Most of the earliest traditions of the *Achera* ascribe the origin of the ancestors of the various totemic groups to certain superhuman beings called *Numbakulla* (= "always existing," "out of nothing"), previously

called *Ungambikula* ("The Native Tribes of Central Australia," p. 388). The cultural progress of the tribe is given on p. 322: (1) In the first period two Num-bakalla, who lived in the western sky, came down to earth and transformed *Inapatua* creatures (i.e., incomplete human beings) into human beings, whose totem names were naturally those of the animals or plants out of which they were transformed. They also performed the rite of circumcision on certain, but not all, of the men, using for that purpose a fire-stick. (2) A period in which the Little-hawk men introduced the stone knife for circumcision and continued the work of transforming *Inapatua* creatures into human beings; they also introduced the four section names (Baldwin Spencer has adopted the suggestion of H. Radcliffe-Brown in regard to the use of the terms "section" and "subsection" to replace "class" and "sub-class," p. 41). (3) Then the Wild-cat men migrated from the south across the Arunta country, introduced the rite of *arilta*, sub-incision, and arranged the initiation ceremonies in their proper order. (4) A period in which certain Emu people changed the marriage system. There are numerous legends of the wanderings of the ancestors of various totem groups, but these are of merely local significance.

*Churinga* are certain sacred objects that are never allowed to be seen by women or uninitiated men (p. 99). The greater number belong to that class of rounded, oval, or elongated, flattened slabs of wood or stone, to the smaller wooden ones the name of "bull-roarer" is now commonly applied. There are two main types of bull-roarer: (1) That of the eastern Australian tribes, from Queensland to Victoria, is associated with tooth avulsion. Among the Kurnai of Gippsland a larger form is called "grandfather," and represents Tündün or male and a smaller one, *Rukut*, the wife of Tündün. (2) That of the central tribes, and probably of the western areas, is associated with circumcision and usually with sub-incision. By far the great majority of these are not used for whirling. Arunta women are told that the noise of the small bull-roarers is the voice of a spirit called Twanyirrika, who enters the body of the boy after circumcision and takes him away into the bush until he is better, when the spirit returns to his wild and inaccessible regions and the boy returns, but now as an initiated man (footnote, p. 202); but this is only a very minor aspect of the *churinga* cult. There is a rich development of ceremonies and a personal relationship between each individual (male and female) and one special *churinga*. The two latter aspects appear

to be absent from the eastern Australians. The *churinga* were first made by Num-bakalla, each with an associated *kūrīna* (the *kūrīna* is the spirit of every individual which continually undergoes reincarnation). The original *churinga* were all of stone, each was split into two, one of which was male and the other female. Later the women's ones were transformed into wood, some of which had a hole bored through them. There are other *churinga* which have no *kūrīna* associated with them. The *churinga* always descend in one moiety of the tribe, but a man may have through inheritance various *churinga* belonging to different totems.

P. Wirz ("Nova Guinea," xvi, 1924, p. 64) found among a mountain people of the upper waters of the Ilim, an affluent of the Mamberamo, in the centre of New Guinea, "power-stones," usually called *pibit* (also the general term for stone tools) or sometimes *ap* (man), which vary in size from about 20 cm. (8 inches) to 70 cm. (27½ inches); they are more or less elliptical in shape and biconvex in section, and are polished, and painted red. When a child is born, the father makes a *pibit* for it, if he has not one as an heirloom from his maternal uncle, as is usually the case. Strength passes from the stone to the child and, indeed, all through life there seems to be some connection between the stone and the soul of the owner. The "power-stones" do not appear to play any part in ceremonies. There is no secrecy about them; though they are always kept in the men's house and remain the property of the men, a woman may borrow one from her husband or brother for the use of her child or for herself. The stones form an inexhaustible source of power mainly for human beings (hence their name *ap*), but may be used to promote the fertility of garden produce. Wirz remarks (p. 65): "One has the impression that here in central New Guinea one is at the beginning, pure and unadulterated, of the use of these power-stones," which he compares with *churinga*.

Wirz ("Marind-anim," 1922, I, ii) says that the exogamous, patrilineal *boan* of the Marind (the people of south-west New Guinea, who are known as Tugeri by their eastern neighbours) is a totemic or local group which is independent of the age-groups, the relationship system, and other social groupings. The totemic conceptions of the Marind arise directly from the myths of the Dema, from a belief in their supernatural powers and potentialities. The Dema created all things; men, animals and other objects who trace back to the same Dema are therefore blood-relatives: they say they are his *Amai* (ancestors, old

relatives). Originally each *boan* had its elaborate fertility ceremonies for men, plants, etc., but, as in the case of the *Mayo* ceremonial, these ceremonies in course of time embraced other totem societies. The magico-religious-social system of the Marind has become greatly complicated by migrations, splittings, and fusions.

There are many close analogies between the cults of the totem-societies of the Marind and various Australian ceremonies, the Mbanbiuma, for example, as A. W. Nieuwenhuis has lately pointed out (*Internat. Arch. für Ethnogr.*, xxviii, 1927, p. 74); these fertility ceremonies also correspond with ceremonies in other parts of New Guinea, in Malekula and elsewhere. The Dema correspond with the Numbakalla, and similarly at the expiration of their activity they withdrew, so far as they had not changed, and betaken themselves into the earth, stones, rivers, sea and sky (Wirz, I, ii, p. 14). The latter originated *churinga* and bull-roarers, and the bull-roarer is associated among the Marind with Sosom, a giant Dema. Wirz (II, iii, 1925, p. 44) says that in the Marind area, as in other parts of Melanesia, bull-roarers are differentiated by their size and tone. On the Upper Bian, as in other areas, they are described as male and female, and especially as "husband" and "wife." Further, S. Lehner (in "Deutsch-Neuguinea," by R. Neuhauss, III., 1911, pp. 410-414) describes the male and female bull-roarers of the Bukana, who live on the north coast of Huon Gulf. It is tempting to regard Twanyirrika as an echo of the novice-devouring monster, such as Sosom of the Marind (Wirz, II, iii, p. 36) and those referred to in the *J.R.A.I.*, L, 1920, p. 238 ff, and by A. B. Deacon, "Folk-Lore," xxxvi, 1925, p. 332 ff.

Attention has been drawn to the migrations of cultures among the Arunta, as these people have been considered by some writers as being extremely primitive, although, on the evidence of the first edition, others, such as Andrew Lang and W. Schmidt, strongly opposed this view. The Arunta themselves admit of culture-bearers who made the backward aborigines into men and taught them various elements of their present culture. There now seems good evidence that this movement came from New Guinea, and that it was mainly a cultural drift.

There are many aspects of this masterly work by Sir Baldwin Spencer that must be left for the consideration of others. In conclusion, it may be mentioned that there are 247 illustrations, four coloured plates and a map; in the first edition there were 133 illustrations: these show more clearly

in the new edition, and all the new illustrations add to the scientific value of the book.

A. C. HADDON.

#### Psychology.

Malinowski.

*Sex and Repression in Savage Society.* By Bronislaw Malinowski. 35 Kegan Paul, Trench, Trubner & Co. 1927. Pp. xv + 285. Price 10s. 6d.

This book is of great interest, inasmuch as it constitutes an attempt by one who has "never been in any sense a follower of "psycho-analytic practice or an adherent "of psycho-analytic theory" to apply the data and viewpoint of psycho-analysis to problems arising from personally conducted studies in the field. Though the detailed matters upon which he touches are fairly numerous and varied, it may be said that the author is concerned with two main questions: (1) What are the psychological conditions associated with mother-right and how do they differ from those associated with father-right? and (2) What modifications in the psychological relationship between the sexes and between parents and children are associated with the origin and development of "culture"? It is clear that the treatment of these two questions must differ somewhat, since the first admits of a more or less direct answer by means of actual observation and comparison of matriarchal and patriarchal societies, whereas the answer to the second can for the most part be arrived at only by a process of inference.

The first question originally framed itself in the author's mind when the reading of psycho-analytical literature stimulated him to reflect on the manner in which the Oedipus complex and other manifestations of the "unconscious" might appear in a community founded on mother-right. This led to a detailed comparison of psychological conditions in (1) the well-to-do classes of Europe, (2) the peasant and proletarian classes of Europe, (3) the inhabitants of the Trobriand islands. As a result of this comparison it appears that the Oedipus complex is not found among the matriarchal Trobriand islanders—or, at least, not in the same form as that in which it has been discovered by psycho-analysts in the patriarchal societies of Europe and America. Significant signs of a corresponding repression of sexual and aggressive tendencies are indeed discovered, but they relate to the sister and the maternal uncle respectively, instead of to the mother and the father as in the Oedipus complex. This leads Malinowski to join issue with Ernest Jones as to the priority of psychological and cultural influences. Whereas Jones, in commenting on this part of Malinowski's work as originally published

in article form, considers that matriarchy has arisen as a means of avoiding father-son hostility, the psychological facts being related to the sociological facts as cause to effect, Malinowski himself holds that the causal relationship is just the opposite and that it is the sociological differences which have brought about the psychological differences. Apart from this matter of causality however, Malinowski is in essential agreement with Jones as regards the fact that matriarchy *does* in certain important ways diminish liability to antagonism between father and son, and he makes some very interesting remarks as to the respective advantages of matriarchy and patriarchy with regard to the avoidance of detrimental inner conflicts connected with the family relationships. Matriarchy is superior in that the mixed feelings of love and of antagonism of the son towards the paternal figure are, as it were, separated out and directed on to different persons—the father proper and the maternal uncle respectively; while the freer sexual life of the young Trobriand islanders also allows of a more continuous and consistent attitude towards the mother. The disadvantage of the matriarchal system is that conflict with regard to the mother is to some extent merely exchanged for a similar conflict towards the sister, for while, on the one hand, the sister is surrounded by the strictest taboos, a man is never allowed to forget or neglect his sister, for whom he is expected to provide and over whose children he exercises authority. There is also the disadvantage that in father and uncle there are two separate sources of authority—a fact which tends to split the family and to produce conflicts with regard to loyalty.

Coming to the second main question with which the book deals, Dr. Malinowski is first concerned with the criticism of Freud's theory of Totemism and then proceeds to sketch his own view of the psychological conditions of the origin of culture. In the critical portion of his book he maintains that "it is easy to perceive that the primeval 'horde' has been equipped with all the 'bias, maladjustments and ill-temperers of a middle-class European family, and then let loose in a prehistoric jungle to run riot in a most attractive but fantastic hypothesis'" (p. 165). Briefly, he holds that the theory is unsound because it endeavours to explain the origin of culture by a process which itself pre-supposes the existence of culture; and that the institution of sacraments, the establishment of laws and the handing on of customs can only take place among people who already possess at least the rudiments of culture. The weakness of the argument in this part

is that it proceeds too much on the "all or nothing" principle; for, indeed, as the author is himself very well aware, the origin of culture is a very slow process, which may even be said to have its beginnings at the infra-human level. But although it seems to the present reviewer that Dr. Malinowski does not succeed in refuting the theory in any form in which it could be reasonably held, he undoubtedly raises some serious problems in connection with the theory which its upholders will do well to ponder—in particular the question as to the precise meaning that is to be attached to the theory; whether there occurred one parricide the effects of which were transmitted by diffusion or "a sort of epidemic" of minor parricides "occurring all over the world, or again, whether, after all, it is (as Kroeber has suggested) only a "just so story," and, if so, in what sense lies its (presumably pragmatic) value.

According to Dr. Malinowski's own view, the processes underlying the development of culture consist in the substitution of a system of sentiments for a system of linked instincts. Suitable sentiments can only be developed within the family, and their development requires that the family bonds shall be preserved beyond the period of immaturity of the younger members of the family. This prolongation of family ties, which is an essential condition of culture, is, however, also the cause of intra-psychic conflict, especially of those conflicts which determine the form of the Oedipus complex; it conditions both the son's incestuous attitude towards his mother and his hostile attitude towards the father. These two attitudes have, however, in their turn, to be overcome on pain of the break-up of the family and the resultant overthrow of culture. Hence the need for inhibition of the attitudes and the consequent formation of the complex.

The essential elements of this view are perhaps not entirely novel, but Dr. Malinowski's treatment is full of interest and suggestiveness; and, indeed, the subject is one that offers many fascinating avenues for exploration, for, as is justly said in the preface, "it is concerned with 'the no-specialists' land between the 'science of man and that of the animal'; hence the present volume does really represent 'from the anthropological point of view, at any rate, . . . a pioneering piece of work.'" J. C. F.

Psychology.

Garth.

*A Comparison of Mental Abilities of Nomadic and Sedentary Indians on a Basis of Education.* By Thomas A. Garth, *American Anthropologist*, N.S. Vol. 29, No. 3, July 1927. Pp. 206-213.

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Following up his work on "The Comparison of the Mental Abilities of Mixed and Full-Blood Indians on a Basis of Education" (*Psychol. Review*, Vol. 29, No. 3), Professor Garth, who has been applying a series of tests to pupils in United States Indian schools, here makes a comparison of the mental ability of 215 reputed full-blood American Indians of "nomadic" ancestry, from south-eastern and plains tribes, with that of 243 full-blood Indians from "sedentary" agricultural tribes in the south-west—Pueblo, Zuni, and Hopi. The tests used were eight group psychological tests of accepted value: three controlled association tests, the opposites test, the genus-species test, and the part-whole test; the free association (continuous); three memory tests, logical memory, rote memory concrete, and rote memory abstract; and one word-building or ingenuity test (the "airlp" test). The subjects in each group, "nomad" and "sedentary," included about the same number from each school grade, and ranged from the fourth grade (which in Indian boarding-schools averages 14.5 years of age) to the eighth-ninth-tenth (18.5 years).

As a result of these tests Professor Garth finds that "in tests of higher mental process, the Indians of nomadic ancestry are on the average 35 per cent. better than those of sedentary ancestry. They are more nearly alike in their performance of memory tests, and more alike in logical memory than in rote memory for abstract words. They are least alike in their performance of the ingenuity test. The differences grow less as the two larger comparative groups can show more education, or education reduces the differences in the test performances between the nomadic and sedentary Indians, but never to less than about 20 per cent. in favour of the nomadic . . . If one questions the value of averages and their comparison as showing real differences, we may put the data to the crucial test of finding how many sedentary Indians attained and exceeded the median performance of the nomadic . . . We find this in the overlapping. By this method we can say that only 32.7 per cent., on the average, of the sedentary subjects did even as well as the median nomadic subject or better than that. That is to say, 67.3 per cent., on the average, of the sedentary subjects, fell below the nomadic median performance."

It should be noted that the Indians brought into this comparison were all pupils in two United States Indian schools, and that the "nomadic Indians" in question are simply children from tribes which were more or less nomadic in the historic past. At the present day these Indians are settled

on reservations, and their wanderings, if they wander at all, are severely limited. Many of them are now agriculturists, and as sedentary as the "sedentary" south-western tribes of the contrasted group.

Before appealing to any inheritance of qualities from the nomad or sedentary experience of these children's ancestors, something should be allowed for the action of present-day circumstances.

The reviewer is inclined to suggest that the comparison to be made here is not between tribes of "nomadic and sedentary ancestry," but rather between tribes whose culture has been broken down and superseded and tribes whose cultural life is still to some extent "integrated."

The reservation life of many Plains tribes, once nomadic, is now a weak imitation of the white Americans'; they have no living body of indigenous tradition, no consistent native *Weltanschauung*, to impart to their children. These children come up to the Government schools unformed, plastic, adaptable; they lend themselves to be moulded by white education. Others of them come from homes with a tradition of two generations of schooling, homes where English is spoken more or less habitually, where life is moulded on the American pattern; for schooling has been enforced on a proportion of the ex-nomad tribes since the 'sixties of the last century, whereas the first determined application of it to the Pueblo Indians of the south-west was after 1884, and to the Hopi after 1900.

The sedentary South-western Indians, on the other hand, have suffered no violent uprooting or change of habits. The white men found them agricultural and have left them so. Their culture, though shaken and encroached upon, still holds together. Their children get a native theory of the world and of living, and begin to be moulded by a body of social and religious conventions, before ever they come under the influence of the Government boarding-school. The mind of the South-western child offers no *tabula rasa* to the white educator; rather, his home education sets up in him a very definite resistance.

It is not surprising that the "nomad" children should not only make a better showing on tests where language is important but should be really superior, grade for grade, in mental alertness under school conditions, because the teaching given to them has been assimilated with less resistance. The ex-nomad reservation child, entering school, steps from a disintegrated and disoriented life to an integrated and consistent one; the South-western Pueblo child comes from an integrated world of which he is consciously a part to a second integration hard to reconcile with the first. He brings, not a

dull mind, but a mind closed and fortified against innovation. Inevitably he must pass through a hampering period of conflict. Perhaps this accounts for the "trace of sullenness" which some competent teachers have noted in almost all Pueblo children in boarding-school.

It is significant that in the last years of school life the "sedentary" adolescents of Professor Garth's experiment are shown to be overtaking the "nomads," reducing their superiority from 61 per cent. in the fourth grade to about 20 per cent. in the eighth-ninth-tenth, and actually excelling them in the test for logical memory.

In using tests of educability, as in this comparison of Professor Garth's, it seems particularly important (after preliminary allowance made for difficulties of language) that the investigator should ask: "How far is the subject free to think along the lines required by the test?" : a question which can not be answered without taking account of the cultural background.

The same criticism might be extended to another experiment by the same careful investigator. Some time ago, Professor Garth published in the *Journal of Experimental Psychology* an interesting record of the colour preferences shown by five hundred and fifty-nine Indian school-children. The order of preference, obtained by arranging a set of seven coloured discs, was red, blue, violet, green, orange, yellow, white; whereas a similar group of white American subjects gave blue, green, red, violet, orange, yellow, white, with blue far ahead of the rest. Here again, before these aesthetic preferences are accepted as "racial" differences,—that is, as due to physiological transmission,—we must surely take account of the subjects' emotional associations with colours. Some of these will probably be found to be not matters of physiological reaction but definitely conventional—part of the social background. To quote a rather obvious example, our traditional European preference for blue—

"green's forsaken and yellow's forsworn,  
but blue's the sweetest colour that's worn,"

says the old rhyme—may quite possibly be associated with the painters' conventional dressing of the Blessed Virgin and, the painters, in turn, may have assigned blue to the Virgin's dress simply because ultramarine was the most costly of mediæval pigments. And if for this colour-test Professor Garth had divided his Indian schoolboys again into "nomadic" and "sedentary," it might conceivably have appeared that the "sedentary" order of preference followed the conventional choice of colours in the South-western Indians' art and ritual and cosmology; which choice, again, is dictated, not by ideal preference,

but by the range of coat-colour in maize, the supply of local mineral pigments, and so on.

Even when the Indian subjects of the experiment are taken as a single group, the results which Professor Garth obtains are suggestive of this cultural background. Most of the South-western tribes have the habit of classifying colours and also of classifying *by* colours; since they divide the universe, theoretically, into six world-quarters, symbolised by six colours suggested by the six varieties of coat-colour in the maize, which is their principal crop: blue (or sometimes yellow) standing for the north, yellow (or blue) for the west, red for the south, and white for the east; black for the nadir, and parti-coloured (the so-called "calico corn") for the zenith. Under these world-quarters and colours they group all the phenomena of their world: trees, birds, mountains, clouds, wild animals, human families, seasons, ceremonies. It is the conventional framework of their thought. Technologically, the ceremonial colours are represented by certain materials: red mainly by red ochre; blue and green (for which a single word serves in most Indian languages) by turquoise, copper ore, gray sands and earths, and the indigo brought in by the Spaniards; yellow by yellow ochre and vegetable dyes; white by gypsum, kaolin, talc; black by hæmatite iron and vegetable dyes. Take now Professor Garth's fourth-grade Indian boys, who may be presumed to be less removed from their original background than the higher-grade scholars. When they were asked to place in order of preference the coloured discs supplied to them, the average sequence which resulted was: red, blue, green, violet, yellow, orange, white. Most of them, as we should expect, put red first—red represented in the concrete by the prized red ochre, used as medicine and charm as well as adornment, sought for over miles of country, traded in minute quantities from tribe to tribe, the cause of forays and wars in old days; "red earth holy to the people," as the Apache call it. But, after this, it looks as if most of them arranged the discs in the order of a conventional classification, putting blue, green and violet together as coming within the range of their single concept of blue-green; then the yellow-orange group; then white. Professor Garth quotes the individual lists of twelve children, and of these the six youngest give:—

1. Age 11. Red. Green blue, purple. Orange, yellow. White.
2. Age 11. Red. Purple, blue. green. Yellow, orange. White.
3. Age 12. Blue. Red. Green. Orange. Purple. Yellow. White.



4. Age 12. Red. Blue, purple, green. Orange, yellow. White.

5. Age 12. Green. Red. Blue, purple. Yellow, orange. White.

6. Age 12. Blue, purple, green. Orange, red. Yellow. White.

(The punctuation is the reviewer's.)

Four out of the six lists, it will be seen, are conventional classifications of the colours presented, and could be read by an Indian in terms of "south, north, west, east" as well as in colours. These Indian boys seem to have arranged the discs much as one sees an Indian arrange a bundle of maize-ears of various colours. It is just probable, then, that Professor Garth obtained, not the boys' order of preference for colours, but the one preferred colour, red, combined with the order in which they preferred to see colours arranged; and that here again a supposedly "racial" test is controlled by cultural factors.

BARBARA AITKEN.

Melanesia: Ethnography.

Ivens.

*Melanesians of the South-East Solomon Islands.* By W. G. Ivens, M.A., Litt.D. London: Kegan Paul, Trench, Trubner & Co., Ltd. 1927. Pp. xx + 529. Price 30s. net.

37

Despite the three hundred years which have passed since Mendaña first sighted the Solomons in his wanderings after the gold of the Incas, much of that island region still awaits competent anthropological investigation. Dr. Ivens deserves our thanks for rescuing from oblivion so much of the knowledge of the decaying culture of this area. There are only three heathen villages now left on Ulawa, the bonito fishing of Sa'a is fast losing its interest, and at Tulagi the author saw a Chinaman building a canoe to sell to natives!

The book is of distinct value as a record of native life. Of economic pursuits such as yam cultivation and canoe building, a good account is given; while of special importance is the detailed and colourful description of the bonito fishing and its ceremonies, including the seclusion of the *malaoku* boys for some time prior to their initiation as fishermen. In the sections on chieftainship, the rites associated with death and burial, the shark cult, the arts of magic, and the belief in the various kinds of spiritual beings, a great deal of valuable information has been collected.

When Dr. Ivens has given so much it may seem ungracious to demand more. But the value of any piece of field work lies not simply in the collection of a mass of data, but also in its formulation of problems and the organisation of data. Method is no less essential in the anthropological field than in any other scientific laboratory. From this point of view

certain parts of the book are not so satisfactory. Much of the material gives the impression of having been hastily put together, with the result that the lack of appropriate setting robs really interesting observations of half their value. The divorce of the folk-lore from its context in social life and technical pursuits, for example, reduces it to a simple collection of interesting stories. The author does not even state whether the tales are only fiction to the natives, or whether they are believed to be true, and perhaps held as sacred. Yet to know this is fundamental to any proper understanding of them.

Serious criticism can also be directed against the treatment of social organisation, the discussion of which is inadequate and full of inconsistencies. A bare list of relationship terms, with some notes on their grammatical usages, the family group, the mother's brother and cross-cousins practically exhaust the subject of kinship. The one page devoted to the *komu* reveals a sad confusion of terminology, linked with defective observation. Thus descent at Sa's and Ulawa is said to be wholly patrilineal, yet the *komu*, the outstanding social group, is defined in Rivers' phrase as a group of persons related to one another, other than by marriage, whether through father or mother. Again, the author tells us: "the *komu*, with its connotation of kinship through father or mother, is the only form of domestic grouping at Sa'a and Ulawa" (61), oblivious of the fact that it is really a kinship, not domestic, group. In any case it is clear from his account (p. 73) that each *komu* is made up of a number of different households. From incidental reference, too, the individual family of parents and young children is plainly distinguishable within the *komu*, but the existence of this fundamental social unit is never explicitly recognised. The term "family" is certainly used, but as defined (61) it refers to the larger social group (*Grossfamilie*). The *komu*, moreover, in addition to being described as "the kindred" and "the family group," is frequently called "the bilateral family," an unfortunate term. Every individual family of parents and children is by nature a bilateral group, since each child belongs to it by virtue of his relationship to both mother and father—by social if not admittedly physiological ties. To use this term for a more extensive social unit such as the *komu* introduces needless confusion, and, as here, obscures the existence of the smaller basic unit.

The final chapter of the book is the least successful. It is an essay after the manner of Rivers, though without that master's skill, to produce evidence of a migration from Indonesia into the S.E. Solomons, and



one feels, perhaps wrongly, that the author has only undertaken it because he conceives it to be demanded of him by precedent. The "cultural survivals" adduced are not very convincing. A story of a woman ghost who cuts up yams for planting is evidence of "a harking back to days when descent at Sa'a and Ulawa was matrilineal" (470), and "the term for the mother's brother which survives in both places bears witness to a former matrilineal state of society" (469). Yet in an earlier statement we are told that at Sa'a and Ulawa "there is no trace of mother-right" (60). Much of the other argument is also a matter of assumption. Thus "of the definite use of 'life-givers' I can furnish no special example . . . There seems no doubt, however, that the use of

"dracæna in religious and magical rites was owing to its blood-red colour, and thus dracæna will rank as a life-giver in the culture of Sa'a and Ulawa" (469). Now contrast with this an earlier descriptive statement. "At Sa'a there is a word *mwasiu*, meaning thin, wasted, fallen away to a skeleton, and the term is applied to a dracæna alongside which men stealthily dropped objects for *si'onga* which took effect upon the victim's body, reducing it to a skeleton" (279). If dracæna is to be called a "giver of life," it must be as *lucus a. non lucendo*!

It is to the merit of the book that the migrational theories of the last chapter do not obtrude themselves upon the true fieldwork record. RAYMOND FIRTH.

## CORRESPONDENCE.

Africa: Gorilla.

Phillipps.

### The Gorilla and Man.

To the Editor of MAN.

38

SIR,—Referring to a letter of mine, dated 15th February, to *The Times*, concerning the carnivorous habits of some gorillas: there has been some hesitation in England in accepting the statements of du Chailly, so many of which have, however, since been verified, regarding the attraction of the human female for the larger anthropoids.

It may be of interest to record the fact that a muHutu woman living at the foot of the Birunga *Mufumbiro* mountains was seized, violated and killed by gorilla in recent years. Her "father" was alive in 1919 and described the incident to me. It took me a year and a knowledge of the *local* language to elicit the facts.

In 1920, a muHutu woman was dragged off in Bufumbira by a male gorilla, of a family which had just been stoned while raiding a banana plantation. He only dropped her about half a mile from the village when closely pursued.

I took down the details from the woman herself and recorded her name and family. The baTwa pygmies, who live a nomad existence on the mountains, were able, after months of hesitation, to recount other specific cases within their knowledge.

Incidentally, in 1920, the professional tanners of the tribe refused to clean a gorilla skin for the purpose of curing it, on the ground that, unlike the (other) apes, he was "a human, though of an abominable kind."

It should perhaps be recollected that the Birunga range has been erupted in

such a way as to form a bridge athwart the Great Divide or Albertine Rift. The gorilla (*beringeri*) has presumably crossed eastward upon it from the steamy forests of the Congo to the intensely cold end of the promontory which juts out into Eastern Africa under the names of Sabinio-Muhavura. There, in the totally different conditions and surroundings, it is not surprising that these few families should have developed very distinct physical and temperamental characteristics.

The primeval tangle of the equatorial forest gives way to clear tree bamboos, and temperate and even alpine flora.

Here the gorilla is emerging gradually from the primeval forest into more open country. In the woods he may frequently be seen moving in the semi-erect position from glade to glade. His tropical food is almost unobtainable. It will perhaps be easier to visualise the environment when it is realised that my first knowledge of the existence of gorilla here was the sight of their footprints at 13,000 feet on fleeting snow.

Here then he has begun to develop at certain seasons a dawning taste for warm flesh. The family is visibly tending to stabilise itself as such, with an *apparent* tendency towards the social organisation of humanity.

It may be noted incidentally that in parts of the baNgala country of the Congo Basin there is an almost standing feud between the men of the tribe and the gorilla on account of the attacks of the latter upon their women.

I am, Sir,

Your obedient servant,

TRACY PHILLIPPS.





FIG. 1.—HOUSES OVER GRAVES IN NANOMEA, ELLICE GROUP.



FIG. 2.—ROCK-CUT BASINS, TANNA, NEW HEBRIDES.

## ORIGINAL ARTICLES.

With Plate D.

Pacific: Ethnography.

Buxton.

**Two Notes from Oceania.** By Patrick A. Buxton, M.R.C.S., etc. 39  
 With Plate D.

During 1924 and 1925 I travelled rather widely in the Pacific, studying various aspects of tropical medicine, and happened to notice two matters, which are of some interest and have never, so far as I know, been recorded.

I. *Houses over Graves in Nanomea, Ellice Group.*

Plate D, figure 1, shows the type of house which is still erected over graves on Nanomea in the Ellice group: the photograph was taken in September, 1924. During that month I landed on all the other islands in the Ellice group (except Nurakita), and on Atafu and Fakaofu, Tokelau group, but I saw no other houses of any sort in graveyards.

The body is buried in the ground and the area over the grave is strewn with white coral, and over it the house is erected. The larger house in the figure was the largest I saw, and was about 5 ft. high to the ridge and 6 ft. long. The native Pastor explained to me that in old days burials had taken place in the houses of the village; but now that it has become the rule to bury in the cemetery the people still erect a little house over the grave. It is interesting to observe this blending of heathen and modern customs in so intensely devout a place as the Ellice group.

It appears that nothing quite like this is on record from any part of Polynesia, though Mariner (Martin) describes the erection of huts in the burying ground (*faitoka*) of a chiefly family, and the strewing of the grave with white and black pebbles.

II. *Rock-cut Basins, Tanna, New Hebrides.*

In September, 1925, I was walking from Sulphur Bay towards Port Resolution, on the south-east corner of the island of Tanna, New Hebrides. At a place where the main path crosses a high neck of land there is a deep gully on the landward side; in the gully and not more than a hundred yards from the path are two large masses of rock (Plate D, figure 2), which have been roughly squared and faced; the rock is red and so soft that it can be scratched with a knife; it is apparently a rotted tuff. In one of the pieces of rock is a circular basin, about 26 inches in diameter, and 8 inches deep; the sides are sharply cut and there are two overflows in the rim, and a few small pit-shaped depressions around the margin. In the other rock is a rectangular excavation about 26 by 9 inches, by 6 or 7 inches deep. Both cavities held water. A small path, which goes down the gully, passes within a few feet of these blocks of rock; but the vegetation round the rocks themselves was not trampled, and it appeared to me that they are not visited or used. I was taken to them by a young man from the Presbyterian Mission, because I was searching for mosquito larvæ in tree-wells and other water containers. I, therefore, discovered them by accident. I was unable to learn the possible use of the cavities, or obtain information about their history.

The existence of these blocks is not mentioned by Humphreys,\* and they were unknown to the Rev. Thomson Macmillan, who has been resident on this side of Tanna for many years.

P. A. BUXTON.

Malay Peninsula: Negritos.

Evans.

**Schebesta and the Negritos.** By I. H. N. Evans. 40

In the present article I traverse some of the statements made by the Rev. P. Schebesta in his recent paper in MAN, 1927, 61. As I am personally a good deal concerned in regard to some of his remarks, I would like to state that

\* Humphreys, C. B., "Southern New Hebrides." London, 1926.

the suggestion that his paper should not be allowed to pass without comment came from Mr. C. Boden Kloss, Director of Museums, Straits Settlements and Federated Malay States.

For easy reference to Schebesta's paper, I will take those statements of his upon which I think that remark should be passed *seriatim*. Page references, unless otherwise stated, are to my "Religion, Custom and Folklore in B.N. Borneo and the Malay Peninsula." He says: "I fail to understand why none of my fore-runners in Negrito research have discovered any genuine tribal names seeing that such do, in fact, exist and are, moreover, in general use." He notes, however, that De Morgan mentioned the Ple, and Vaughan Stevens, the Kensiu. He might also have added the Kintak and the Kintak Bong to Vaughan Stevens's credit (*vide* p. 36 of my "Papers on the Ethnology and Antiquities of the Malay Peninsula," just published). Annandale and Robinson have a long article on the Jehehr (the Malay version of Jehai) in "Fasciculi Malayenses," while they have the name Pô-Klô for Schebesta's Ple. To come now to my own work, if Schebesta will take the trouble to look at pp. 144 to 146, he will find the tribal names Menik Kaien, Kintak, Kintak Bong, Semak Belum (= his Sabubn), Jehai, Kensieu, Menik Semnam, Batek, Pleh and others. I have also pointed out that a tribe may have at least three names, sometimes more. Thus, there is the name by which it calls itself, that by which the Malays call it, and that, or those, by which it is known to various surrounding tribes. Thus, Schebesta's Sabubn (my Semak Belum) are called Lanoh by the Kintak Bong, Sakai Jeram (Sakai of the Rapids) by the Malays. Considering that, on Schebesta's arrival in Malaya, I put a copy of my book into his hands, as well as all papers of mine which had appeared to date in the *Journal* of the Federated Malay States Museums, it is curious that he should make the statement that no tribal names for Negritos had been recognised before he made his researches.

Schebesta also says that "the Malays are quite unaware of any tribal designations among these aborigines. By chance it so happens that a tribal difference is, in fact, indicated. . . ." I do not think that altogether correct. The Malays who live in touch with the Negritos do, in many cases, realise the existence of definite groups, but they do not generally use the name by which the group calls itself, but invent some descriptive term for it. The Malays of Temengor, however, do sometimes call the Jehai Sakai Jehehr, while the Bateg of the Cheka River are known to the Malays as Batek.

Another point which is, perhaps, worth attention is with regard to the origin of the terms Semang and Pangan, the latter of which Schebesta persists in spelling Panggan, though he already had Skeat's and Blagden's good examples to follow, not to mention mine! He says: "Semang and Panggan remain as recognised terms and demand an explanation, and I sought for one in vain in the literature here available." I agree that, so far, the term Semang has not been satisfactorily explained, but with regard to the name Pangan, granting that Wilkinson's Malay Dictionary is correct, which I have followed in my book (p. 144), it means "extensive tracts of forests." Orang Pangan, therefore, is equivalent to Orang Utan. The meaning of Sakai, too, is as "followers, retainers, dependents; a name also given to aboriginal tribes as being theoretically races subject to the Malays." The word is used in the sense of followers in old Malay romances. A curious point to which Schebesta has not drawn attention is that, though the Negritos detest the term Semang being applied to them, since they think it derogatory, they do not, as a rule, object to being called Sakai and will, indeed, often say that they are Sakai if questioned, as they have got the idea that a Sakai is quite a superior person. The true Sakai (of the anthropologist), to whom the name Semang is not applied by the Malays, loathe the name Sakai as much as the Negritos do that of Semang, and the

Malays, though referring to them as Sakai among themselves, when they wish to be polite, address them as Hill People (Orang Bukit) or by some such other non-objectionable term.

Schebesta makes one suggestion (already made long ago) with which I—having followed it persistently of recent years—thoroughly agree, *i.e.*, that the terms Semang and Pangan be replaced by “Negrito,” while the term Sakai, in default of anything more satisfactory, should be retained. I also agree that all Malay names for aboriginal tribes should, as far as possible, be abandoned.

Schebesta describes his journey through Malay as perilous. This seems rather a strong word to use. I cannot see that it was more risky than those often undertaken by persons whose work takes them to the jungle. There are, of course, the usual chances of illness or accident, and such work, even under the most favourable circumstances, entails a certain amount of discomfort. I. H. N. EVANS.

### Africa, East: Technology.

Elliot Smith.

**The Boats on Victoria Nyanza.** By G. Elliot Smith, M.D., F.R.S.

41

My attention has just been called to the interesting discussion by Mr. James Hornell in MAN, 1928, 1, on the boats in East Africa. Mr. Hornell rightly criticises the error I unwittingly introduced into my copy of H. M. Stanley's drawing (“Through the Dark Continent,” Vol. I., p. 451) of the type of boat used on Victoria Nyanza, which, to use his words, “created a fictitious parallelism between the African and the Scandinavian type” (the rock-cut representations of the Bronze Age at Bohuslän).

In frankly admitting my error, I should like to explain how the mistake arose and why it cannot bear the implications Mr. Hornell assigns to it. So far from destroying my argument the correction adds to its strength and cogency. Not only the picture in Stanley's book was misleading, but also the description given by the late Sir Harry Johnston (“Uganda Protectorate,” Vol. 2, p. 659) of the way in which the boats were made. He concludes his report in these three sentences:—“The prow and stern are finished by another hollow half cylinder of wood stitched to the end of the planks. The prow end of the keel is also strengthened by a long bent pole with a backward twist, being securely fastened to the keel. The top of the prow is generally ornamented with a pair of horns, and it is steadied by a stout rope being carried tightly from the uppermost point of the prow to the nose or beak of the canoe.”

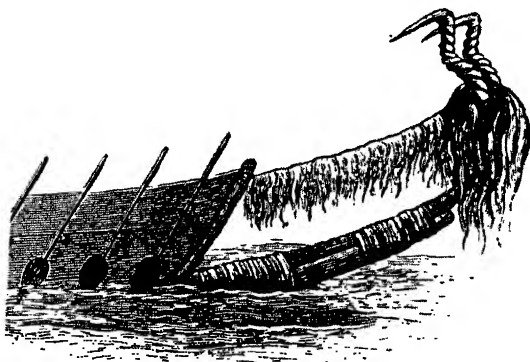


FIG. 1.—PHOTOGRAPH OF PART OF H. M. STANLEY'S WOODCUT OF THE FRONT END OF A BAGANDA CANOE. (Facing p. 450 in Vol. I. of “Through the Dark Continent.”)

In the passage I have put into italics it will be noticed that the word “prow” is used in a different sense from that of the first of the three sentences quoted. As Stanley's cut (Fig. 1) showed no trace of any connection between the horned head and the keel but what we now realise to be a decorated string connecting the upturned keel to the hull, I did not detect the ambiguity in Sir Harry Johnston's use of the word “prow,” but imagined it to confirm the impression I got from Stanley's illustra-

tion, which I now realise to be mistaken. The correction of this error brings the boat represented by Stanley into conformity with the types of ship depicted by other writers on Victoria Nyanza and the upper reaches of the Nile, and also with those represented on the ivory handle of the Gebel-el-Arak knife (which is Protodynastic Egyptian). While this correction undoubtedly detracts from the closeness of the likeness between the Uganda boats and those represented in the Swedish rock-carvings of the Bronze Age, it does not destroy the homology between the two ships. The points of most importance are the use of a ram's head and the custom of prolonging the keel far in front of the hull of these vessels. The fact that the ram's head is in one case on the upturned end of the keel and on the other (in which the keel is not turned up) is fixed instead to the prow of the vessel is a matter of relative unimportance compared with the fact that both ships have the distinctive features of the prolonged keel and a ram's head somewhere on the front of the boat.

Instead of weakening my argument these facts really strengthen it by bringing the Scandinavian type into closer agreement with the Mediterranean models and so forming a more natural linkage. In 1912 Dr. Müller-Wismar ("Austroinsulare Kanua als Kult- und Kriegs-Symbole," *Büssler Archiv*, II, 1912, footnote 1, p. 239) claimed that the boat used on Victoria Nyanza was linked by the Nilotic craft with Mediterranean vessels. He doubted whether they could be connected with the boats of Madagascar. While Mr. Hornell, apparently forgetting that the type of vessel under discussion is represented on the Gebel-el-Arak knife (circa 3400 B.C. in Egypt), wants to claim an Indonesian origin for the canoes on Victoria Nyanza, Dr. Müller-Wismar will not admit that there is any connection between them and Oriental (Indonesian) canoes of a similar form. The type found on the Upper Nile and Victoria Nyanza is a distinctive modification of an Egyptian prototype, other variants of which were used in the Mediterranean and Red Seas, and from these waters spread respectively to the Western littoral of Europe and to the Indian Ocean and the Far East.

When the whole of the available evidence is set forth we have a *prima facie* case for the remote antiquity of this type of vessel on the Nile and of its diffusion east and west—to the Far East and Northern Europe. Moreover the peculiarities of the Indonesian vessels so clearly described by Mr. Hornell can be more easily explained as derivatives of the Northern (Mediterranean) rather than the Southern (Baganda) modifications of the Egyptian prototype.

G. ELLIOT SMITH.

## Anthropology, Physical.

Shaxby: Bonnell.

**On Skin Colour.** By J. H. Shaxby, D.Sc., F.Inst.P., and H. E. Bonnell, B.Sc. (from the Physiology Institute, Welsh National Medical School, Cardiff).

42

The absorption of radiant energy by the skin has long been recognised as of physiological importance, and a good deal of work has been done on such subjects as the absorption of ultra-violet light in connection with photo-therapy. But the general question of the reflecting and absorbing powers of skin for light of different colours seems to have been very little investigated. In particular, we know very little of the exact differences in this respect between the skins of different races; we have no clear quantitative measures of skin-colour.

The authors have, therefore, begun a research upon this question; it is intended eventually to deal with a wide range of wavelengths, but we felt that it was desirable to clear the ground first by making measurements upon a large number of subjects for white light and for the longer and shorter parts of the visible spectrum. The present paper gives some account of the method adopted in this survey and of the preliminary conclusions reached for the skins of "whites" and those of West African and West Indian negroes and half-castes.

As it is clearly impossible to measure directly the absorption by the skin of the living subject, the diffuse reflecting power was measured; the proportion of light absorbed is obviously unity, less the proportion reflected. This, of course, only gives information as to the total absorption and can, unfortunately, tell us nothing of the treatment of the light by the different layers of the skin.

We first thought of using rotating Maxwell discs (the colour top) to match the light reflected by skin, but rejected this method for the following reasons:—

(1) The intensity of light reflected by a disc of any colour is not readily expressed in terms of wavelengths, and our purpose was not merely to make colour matches of different types of skin.

(2) The light reflected by colour discs is not of pure spectral hue: for example, the light from a yellow disc is mainly red and green. Thus the proportions of the different discs giving a colour match afford no direct information as to the amounts of light of different wavelengths which are concerned. Further, considerable correction is necessary for the light reflected by the black disc.

(3) The surface of the discs is very different in texture from that of skin, so that errors are introduced and agreement between different observers is poor, because of the necessity of judgment in matching tints as opposed to matching merely brightness.

We therefore adopted a photometric method, illuminating the skin and comparing the amount of light, white or coloured, which it reflects with the amount reflected by a standard matt white surface. The coloured lights were obtained by transparent filters, which have the advantages that they can be selected to transmit any part of the spectrum and that they are accurately reproducible, so that specification of the make and reference number of a filter enables other workers to repeat any observation.

For this preliminary survey two Wratten (Kodak) filters were used, Nos. 25 and 58. The former transmits red light from the end of the spectrum to wavelength  $5,800 \text{ \AA}$  (orange-yellow); the latter transmits a green light from about  $5,900 \text{ \AA}$  to  $4,900 \text{ \AA}$ , of effective mean wavelength  $5,300 \text{ \AA}$ .

The measuring apparatus was a Trotter photometer, *P*, arranged as shown diagrammatically in Fig. 1. The light to be measured falls on a white screen, *SS*, with a central aperture, *A*. Through this aperture shines the light from a standard lamp after reflection from a diffusing screen, both inside the photometer case. The brightness of this emerging light can be controlled widely by rotating the diffusing screen by means of a knurled head, *H*, on the top of the case. This at the same time turns a pointer indicating on a circular scale the foot-candles of the illumination. If, then, the pointer is turned until this light matches in brightness that from the source to be measured, the scale reading denotes also the foot-candles due to the latter. The light to be measured in our experiments is that diffusely reflected by a patch of skin, *X*; the region to be examined is placed against a circular hole cut in a screen, *ss*, covered with black velvet on the side toward the photometer. A standard area at a fixed distance is thus always used, and is illuminated strongly and evenly by a second lamp, *L*, whose rays fall on the skin area and on a narrow ring of the velvet around the hole. The observer views the photometer screen, *SS*, as reflected in a thin half-silvered mirror, *MM*; the large black screen gets in the way of a direct view.

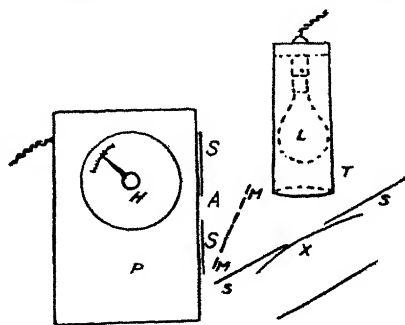


FIG. 1.



Observations are made successively with white, red and green light, first with the given area of skin, next with the standard matt white surface, and, finally, with the hole in the velvet screen left empty, to allow for the small quantity of light reflected by the ring of velvet on which light falls in the two previous measurements. The mean of three readings is taken for each test. The whole process is rapid, and different observers obtain good agreement with one another. The ratio of the corrected skin readings to the corrected matt white surface readings measures the relative reflecting power of the skin, the reflection coefficient required. As need hardly be pointed out, a high value of this coefficient for white light denotes a fair skin, for red light a florid skin, for green light a sallow skin.

A sample set of readings thus runs somewhat as follows :—

	Foot-candles.		
	White.	Red.	Green.
Skin - - - - -	0.61	0.82	0.45
White standard - - - - -	1.12	1.14	1.10
Blank - - - - -	0.07	0.07	0.07
Reflection coefficient - - - - -	0.52	0.70	0.37

The figures of the last row are calculated thus : For white light the ratio is

$$\frac{0.61 - 0.07}{1.12 - 0.07} = \frac{0.54}{1.05} = 0.52, \text{ and similarly for red and green.}$$

Relative ruddiness of tint is conveniently measured by the ratio of the red coefficient to the green coefficient; we term this the "red-green ratio." At any stage of general fairness or darkness of skin one finds considerable variations in floridity as measured in this way. The records of reflection by the cheek show that a negro may be "ruddy" or "pallid" between about the same limits as a European.

The conclusions which follow are based on 200 observations of Europeans, nearly all Welsh or English, 35 of negroes and 32 of half-castes.

*White Light.*—Among European subjects there are, of course, wide variations in reflecting power, but among those so far examined the reflection coefficients lie between the extremes of 0.5 and 0.8 for the inner aspect of the arm, 0.4 and 0.6 for the cheek. The regions examined, in order of increasing reflecting power, are palm, back of hand, forehead (0.45), cheek (0.50), outer side of arm (0.58) and inner side of arm (0.66). The figures represent rough average values of the coefficient.

For negroes the order is quite different; as is well known, the palm is comparatively pale. The corresponding list is back of hand (0.17), forehead (0.18), outer side of arm (0.19), cheek (0.20), inner side of arm (0.24), palm (0.26). Still less than in the former list should any value be attributed to the exact figures given. The ratios of reflection coefficients of negro to European skins are roughly 60 per cent. for the palm, 30 per cent. to 40 per cent. for the other regions measured.

Half-castes, obviously, give intermediate values, widely varying among themselves, and the order of reflecting powers for different regions may approximate either to that of the European or of the negro. It should be added that the half-caste subjects were chiefly children.

*Red and Green Lights.*—With the total reduction in reflecting power in passing from fair to dark skins goes, of course, a decrease for both red and green coefficients. This is seen in all cases. For green light the change is practically parallel to that in the total (white) light change (Fig. 2). For red light the tendency seems to be for the fairer subjects to show relatively less red-reflecting power. In other words, the swarthier skin is also slightly the redder skin. This is indicated in the graph of Fig. 3, where it will be seen that the red coefficient does not increase quite so fast as the general white coefficient in passing from the darker members of the population on the left side of the plot to the fairer members on the right. The general trend

GREEN REFLECTION COEFFICIENT.

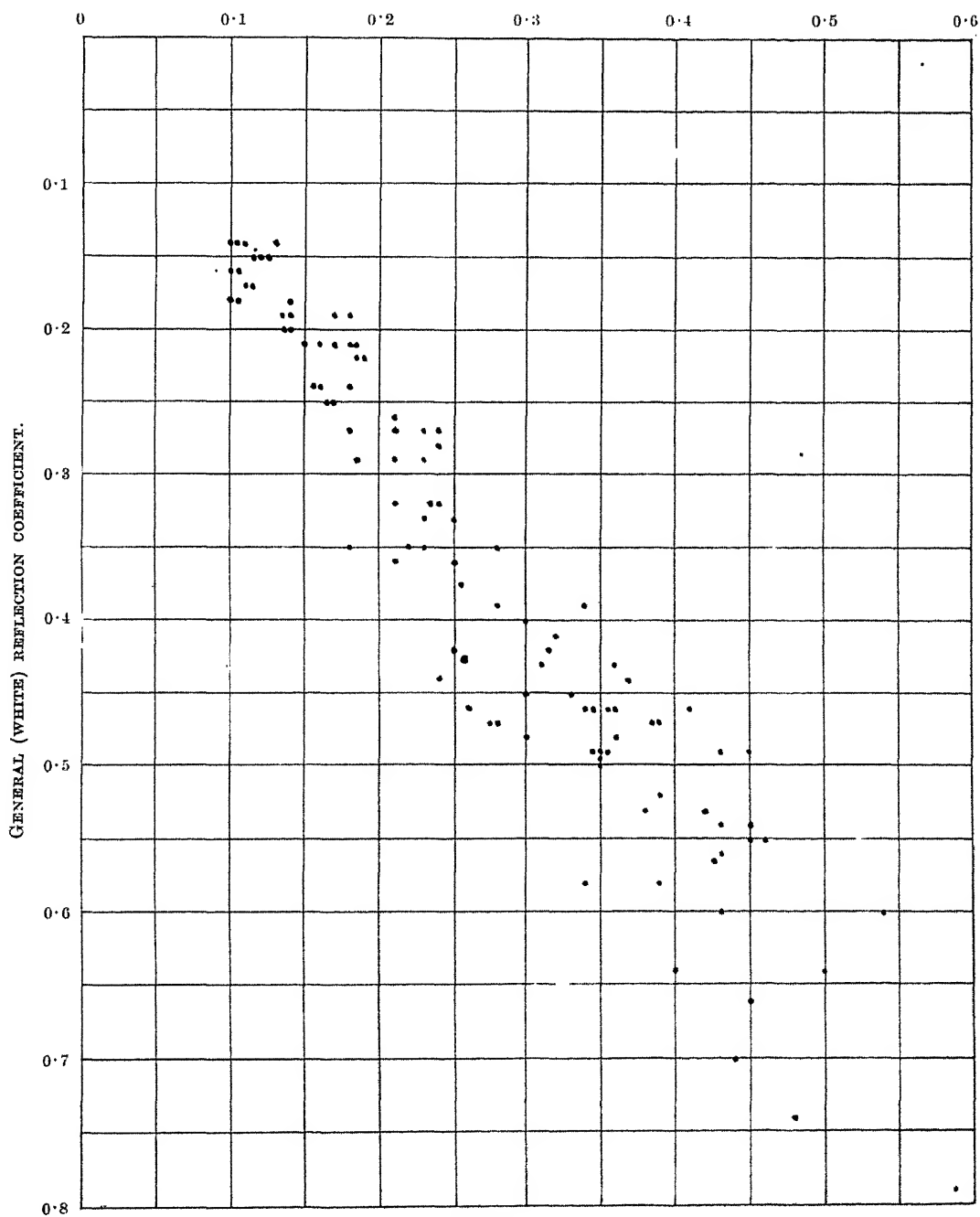


FIG. 2.

of the mean curve is a little convex upwards, while that of the corresponding green-white graph of Fig. 2 is practically straight. The same thing is shown if the red-green ratio is tabulated against the white reflection coefficient. The change

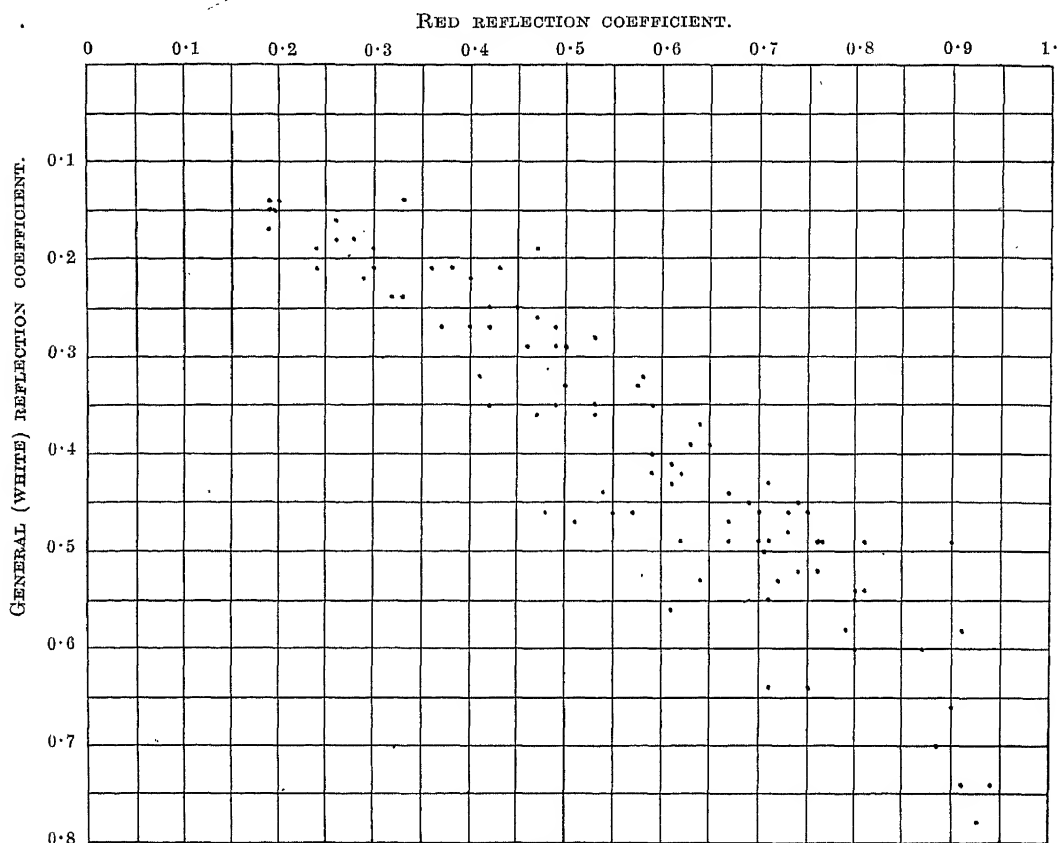


FIG. 3.

is, however, small; the correlation coefficient between the two quantities just mentioned is only  $-0.16$ , with probable error of  $0.09$  for the cases shown in the graphs, though it is somewhat greater for the whole mass of data obtained.

An interesting point is that there is no fundamental discontinuity in these respects between "whites" and negroes. The arrays of points in the graphs lie evenly about a straight line in Fig. 2, a curved one in Fig. 3. There is no break anywhere: no one could tell from an inspection of the graphs that below the ordinate of white coefficient about  $0.35$  there is not a single negro, above it not a single pure European.

The data for Chinese subjects are at present insufficient to warrant any conclusions.

*Summary and Conclusions.*—The reflecting power of human skin has been measured for about 270 subjects, European and negro, by comparing the light reflected under standard conditions by a patch of skin with that reflected under the same conditions by an equal area of a white matt standard. The reflection coefficient thus obtained has been determined for whole white light and for red and green lights.

There appears to be no difference in type between the reflection of "white" and "coloured" skins, negroes in this respect differing from Europeans only in the amount of melanin pigment in the skin.

J. H. SHAXBY.  
H. E. BONNELL.

Egypt: Games.

Hornblower.

**Wrestling: India and Egypt.** By G. D. Hornblower.

43

An account was published, in the *Times* of 2nd March, of modern wrestling in India, which included a description of the ceremonial acts preceding a bout. In most places each wrestler "breathes a silent prayer, then touches the sand three times and lifts some of it to his brow, after which he leaps up and down in the open, slapping his thighs with resounding smacks. Some only perform this ritual just before their own bout; others go with the party from their own wrestling school and worship in unison. The experts go through an immense amount of preliminaries, even after the formal shaking of hands. They do a press-up to improve their own muscles, and a squat or two to relax their legs, and smack their biceps before facing each other in a crouching position, left hand on thigh and right hand outstretched to grip. Whenever, however, the spectators are expecting a start, they get up, walk round the ring, and face again with right hand on thigh and left out. The crowd will not stand such nonsense from beginners, who are expected to get on with it quickly; but the great men refuse to be hustled. . . . The battle is not over until both shoulders are touched down, and it is surprising how wriggles and contortions will extricate a man."

A striking parallel to this was witnessed by me in Egypt one night over thirty years ago. It was in the month of Ramadan, when a great part of the populace is out at night to enjoy the various festivities offered them throughout that month—dancing, singing, circusses, side-shows of all kinds, in fact "all the fun of the fair." In a marquee, set up in the lower part of Alexandria, benches, on a slope, surrounded the ring, which was thickly strewn with sand. Here wrestling was exhibited by men paid as professionals; various modes were announced—Greek, Turkish, Frankish, Egyptian—though there did not seem to be much difference between them and the catch was always, I think, catch-as-catch-can; also, to win, the adversary's shoulders should both be in close contact with the ground, as in India. The wrestlers were naked but for a very short pair of leather drawers; I do not know if they were rubbed with oil, but their skins shone as if it were so; they often gripped by the drawers.

The most interesting feature was the preliminary ceremony: each antagonist said his silent prayer, the "Fâtiḥah" (the opening chapter of the Qûran), and touched the earth with his forehead three times, picking up some sand; he then slapped his thigh loudly and started stalking round the ring, most swaggeringly, with exaggerated strides, uttering loud cries, apparently of defiance, in fact champing round in real fee-fi-fo-fum style. Sometimes one of them, seeing a friend among the onlookers, would go to him and, placing his head against his breast or shoulder, repeat with him the Fâtiḥah, thereby getting a share of his "barakeh" or luck, "mana" (this action is called "tebarrak"). After this preliminary performance, usually rather long, the wrestlers faced each other, springing up and down on their thighs, calling out, and then with a final shout, grappled vigorously. (There was also sword-and-buckler fighting, of a rather anæmic nature.)

Though I have since visited many booths in moolids and fairs, this was the only time I saw wrestling; it may survive to the present day, but, what with cinemas and other new attractions, it has quite likely died out, like the "Kara guz" shadow-plays, so popular in the days of Lane—this I have only seen once, at the moolid of Sidi Ibrahim at Desuq, though often seeking for it; the tent was very small and squalid, the attendance scanty; poor "Kara guz" was fading away. There was much and constant obscenity in these shadow-plays and to-day they would be stopped by the authorities, as have been the public performances of the belly-dance and of the slanging competitions ("nukta") of unspeakable grossness that

served as interludes to the dances, all of which were in full popularity fourteen years ago. Their place has been taken by singing and the native theatre, but of course the cinematographs overshadow all old vehicles of amusement.

G. D. HORNBLOWER.

## REVIEWS.

Archæology.

Allcroft.

*The Circle and the Cross : a Study in Continuity.* By A. Hadrian Allcroft. In 2 volumes. Vol. I. The Circle. Pp. x & 370 with 4 plates. London : Macmillan & Co., Ltd., 1927. 12s. 6d. net.

44

The material now set forth in book form has been published already in the *Archæological Journal* (Vols. LXXXVII.-LXXX.) and is well known to most students of British antiquities; but the content of Mr. Allcroft's essay is such that an appeal not only to the specialist but to the reading public is justifiable, and there is little reason to doubt that the two volumes, of which the first is now before us, will be warmly and widely welcomed. It is to be observed, however, that the reprint has seemingly been made from moulds, so that the author has had little opportunity of revision of the text in those passages where the results of recent research affect his argument; indeed, the chapters reappear with little discoverable alteration.

An attempt to establish the origin of the word *church* has prompted the wide enquiry of which the present volume is but the first instalment, and Mr. Allcroft has dealt faithfully with each item in the long series of problems that confronted him. Beginning with the circular churchyard, the author argues that this form of garth, which he courageously derives from the round barrow, is, in reality, the traditional form, and, so saying, he is led to ask whether the word *church* may possibly be connected with this circular garth. Here he promises us a ready explanation, withheld from this first volume, of the obvious difficulty presented by Wales where the circular churchyard is often to be found in what is plainly an *ecclesia* (*eglwys*) province. Turning back to the subject of the round barrow, the author also promises to prove conclusively that the customary burial-places of the earlier Christians of these islands were as frequently barrows as were those of their pagan forefathers; but, immediately, he is content to give a survey of the various types of pre-Christian barrows. The long barrow, being non-circular, must be assigned to a civilisation and religion earlier than those of which the round barrow was symbol, the round

barrow, in fact, being proof that the circle was to the builders of the barrows the symbol of sanctity and the venerated mark of a *locus consecratus*. It is only natural, then, that this "cryptic circle" should, in the course of the centuries, be expressed in such divers forms as the bowl, bell, and disc-barrow, and that finally the sacred sepulchral ring might stand alone, either as an earthen ramp or a circle of stones.

Extending the range of this enquiry, the author turns to the Achæan moot, encouraged thereto by the *ἑρὸς κύκλος* with its smooth stone seats as described in the *Iliad* and the *Odyssey*, and thence to Rome where the *Circus Maximus* represents, in his view, a primitive circus that was nothing other than a moot. From Rome Mr. Allcroft passes to Denmark, where the circular *ting*, a form of moot that he tells us is certainly developed from a barrow, is welcome material for his purpose. From Denmark we are led easily to Scotland, and this provides occasion for a critical survey of the famous stone circles of Aberdeenshire, where, in Mr. Allcroft's opinion, we meet with the exact replica of the Homeric moot; to this is added an account of the earthen *circei*, or amphitheatres, of Britain, and also of the stone circles, this last section being followed by a discussion of the date of these rings. In so far as these British monuments are concerned, Mr. Allcroft writes with the full authority of a recognised expert, and gratitude is due to him for this careful and penetrating study, which, it may be added, is well illustrated. The decision that in general the stone circles are the moots of Brythonic invaders, and as late in date as the La Tène period, stands, however, at the moment small chance of acceptance, but it may be that in the argument under this heading Mr. Allcroft is his own worst enemy. It is clearly an advance to know, as many will now readily believe, that some circles may have been moots, and to know, as has often been suspected, that some circles may be of relatively a late date; but the author's urgency to state this purpose and age as a rule to be applied generally is likely to repel many readers from a fair consideration of his views. For it may safely be said that, although we know

too little, as yet, about our stone circles to attempt a final verdict on their date and use, at least we know just enough about them to test any possible verdict with some severity. In the present instance, to take an easy example, those who know the masses of archaeological material from the surviving Er Lanic circle think they have good grounds for believing that the ring was set up in the early Bronze Age, and they could hardly be persuaded that the thick litter of sherds and broken stone implements found within it was the carpet of a moot. Moreover it is to be doubted if there are really as many demonstrably non-sepulchral stone circles as Mr. Allcroft believes. Certainly, it is improper to claim the supposed stone circle on Mag Slecht (p. 275) as one of these, for the late and dubious aetiological legend to which the author is here referring might as easily apply to a sepulchral circle as to one used only for the purposes of worship and assembly. As for the small earthen circus, it may likewise be asked if the moot-explanation is in reality all-sufficing, why, for example, in the instance of the Ysceifiog circle and barrow in Flintshire should the La Tène moot hypothesis be preferred to the simpler reading of the remains by Dr. Fox (*Arch. Cambr.*, 1926, p. 68) as a burial-place of about 1800 B.C.? If there is room for doubt here, it is likely that there will be two opinions as to the real purpose of many of the "moots" described by Mr. Allcroft, even though sensible archaeologists will not hesitate to acknowledge that the author's interesting and legitimate suggestion is one that cannot henceforward be ignored. As a general criticism in this section it is to be noted that Mr. Allcroft has strangely neglected the rich material in early Irish literature concerning the holding of public assemblies, and the studies that have been devoted to the manner of these meetings and the places where they were held. It is doubtful if the sacred circle is so plain here as Mr. Allcroft would like to find it, but there is support for the *locus consecratus* theory in the common choice of the graves of great heroes as the meeting-ground.

From the circles and the moots Mr. Allcroft turns to their builders, and in two chapters he discusses the Nordic peoples and the Cimbri, giving us, in passing, an account of druidism. It must be confessed that the ethnological section of the enquiry is the least satisfactory portion of the book, for Mr. Allcroft's exciting views must be better documented and better defended if they are to be taken seriously. In any event, it would be an advantage to know what is in the author's mind when he uses the term Brython. Why are disc-barrows

in Ireland the work of Brythonic invaders of that island, and who were these invaders? Ptolemy's Brigantes and Menapii might be a reasonable answer to the last part of this question, though it is difficult to guess why they should be thought to have made disc-barrows; but the author's view seems rather to be that the Fir Bolg (p. 328) were Brythons of the 1st century A.D., and that the Tuatha dé Danann (p. 332) were also Brythonic. Mr. Allcroft terms these last Brythonic forerunners of the Danes who invaded Ireland possibly as late as the 4th century A.D., and who were, he adds, of the same stock as the Picts and the inhabitants of Picardy. But it has been clearly shown, and it is now accepted, that the Tuatha dé Danann were peoples of pagan mythology, and, since the tales of their doings are woven into the legends of the Ulster cycle, thought to relate to the 1st century B.C., it is next to impossible to accept them as historical Celtic invaders of the Christian era. Similarly, it is hard to think of any intelligible reason for calling the pre-Celtic Fir Bolg a Brythonic folk. As regards the Picts, though few will agree that their name is to be detected in Picardy, it is to be acknowledged that Mr. Allcroft has argued with force against the current opinion, and his remarks should be studied in conjunction with Professor W. J. Watson's important article (*Gaelic Society of Inverness*, XXX., 1919-22, p. 240).

In the last chapter the author makes the interesting suggestion that the words *cruc* (Welsh) and *cruach* (Irish), meaning heap or hill (moot-hill, says Mr. Allcroft), together with the Latin *circus*, shortened to *circ*, have given us by such stages as *cyrc*, *kirk*, *cyric*, and *cyrice*, the modern word *church*. In sum, Mr. Allcroft believes that the pagan symbol, the circle, lies hid in fossil form in the present name of the Christian place of worship, an hypothesis that is ably and plausibly argued, even though philologists may still prefer the much-debated *Tò Κυριακόν*, the *Lord's house* from *κυριακός*, belonging to a lord or master, as the true source of the word church. In fact, the theory that derives church from circus has already been discussed and rejected, and it is to be feared that phonetically *cruc* and *cruach* do not suggest themselves as conspicuously well-chosen allies for the discredited *circus*. But, at least, it is certain that the verdict of the philologists on this point will be awaited with interest, and so too will Mr. Allcroft's second volume, wherein, it is to be supposed, we are to hear the story of Christianity's long struggle to square this pagan circle.

T. D. KENDRICK.

## Archæology.

Peake: Fleure.

*The Corridors of Time* (I. *Apes and Men*; II. *Hunters and Artists*; 45 III. *Peasants and Potters*; IV. *Priests and Kings*). By Harold Peake and Herbert John Fleure. 7½ in × 5½ in.; pp. vi. 138, 47 ill. + vi. 154, 79 ill. + iv. 152, 63 ill. + 208, 115 ill.; 4 vols. Oxford: At the Clarendon Press, 1927. Price 5s. net each volume.

The field covered, or attempted to be covered, by modern anthropology, is so large; its hypotheses and interpretations so complex and far-reaching; and its controversies so fundamental, and, according to some views, so irreconcilable, that Dr. Marett spoke very truly when he said, in his own admirable pamphlet in Benn's Sixpenny Library, that anthropology will be kept sane only by being popularised.

Of the present popularisation it may confidently be said that it will not merely help in the preservation of sanity, but that it will form a definite contribution to knowledge. It is impossible that the authors should succeed in pleasing everybody in covering so large a canvas. But Messrs. Peake and Fleure handle their innumerable facts with masterly ease, show a true sense of proportion in the allocation of space to the several elements of the subject, and, above all, are quite astonishingly impartial in the presentment of rival theories. Where they do lean more decidedly to one side rather than to another, as in the difficult problems of Ice-Age and Mesopotamian chronology, they do so only because it was a matter of sheer necessity to impart some coherence into the complicated account of man's origin and early days.

It is to be hoped that "The Corridors of Time" will in due course be revised and reissued, in less generous *format*, as a single volume. The authors may then consider it advisable, when introducing technical terms, to add some explanation of them; at present a good many pages will prove incomprehensible to the layman. The illustrations should also be sub-edited, with a view to providing the fullest particulars always below each figure, and not sometimes there, sometimes in the table of contents, and sometimes in the list of acknowledgments. Misprints are few and trifling; but in "Hunters and Artists" (p. 71, in the description of the old woman of the Grotte des Enfants, Grimaldi) for 6½" read 62½", and in "Priests and Kings" (fig. 68, p. 115) the chart of the early civilisations of the Ægean region mistakenly shows Early Cycladic II in two parts.

THEODORE BESTERMAN.

## Africa: Ethnography.

Lindblom.

*Jakt- och Fångstmetoder bland Afrikanska Folk.* Av Gerhard Lindblom. 2 vols. Stockholm: V. Petterson, 1925, 1926. 9¼ × 6¼. Pp. 138, 158.

Dr. Lindblom's book is a model of careful and thorough study in a special department of ethnography. It is surely as nearly exhaustive as it is possible for a human work to be. It is based on personal examination of specimens in the Stockholm Ethnographical Museum, the British Museum and the Berlin Museum für Völkerkunde, supplemented by information obtained from Copenhagen, Frankfurt a/M. and elsewhere; and the author seems to have overlooked no relevant item in the voluminous literature of African travel, his lists of references filling eight pages in the first volume and ten in the second. To the Swedish text is added, in each case, a "retrospect" in English: that in the first volume gives a brief but admirable summary of the contents, but the second must not be relied on as a substitute for reading the book, "being simply intended to point out some items of more general interest." This volume contains a feature wanting in the first—an English version of the table of contents—which will be of considerable assistance to those unable to read Swedish easily.

Dr. Lindblom's original intention was to arrange his subject under the headings of the various hunting and trapping methods employed, irrespective of the kind of animal in question. He found it, however, more convenient to treat each animal separately, without, however, rigidly adhering to this method. Thus he begins with the various ways of hunting the ostrich, the crocodile, the hippopotamus, the giraffe and the elephant; but, along with these, he has chapters devoted to disguises used in hunting (as by the Bushmen, the Somali, the Nupe, etc.) and to the use of the boomerang, the throwing-stick (*kastklapp*) and the missile club (*kastklubb*). The second volume describes the modes of killing or capturing apes and monkeys, lions, leopards and other carnivora, pigs, various rodents, and birds. Both gorillas and chimpanzees are shot and trapped by the Pangwe and other tribes in West Africa, by the Babembe of Lake Tanganyika, the Wambutu pygmies and others; while, on the other hand, the inhabitants of the Kion basin never hunt them, and the same is the case with the Bakwiri of the Cameroons, and, to a certain extent, in Liberia. The author is no doubt right in thinking that the introduction of firearms "has contributed to make the negroes of West Africa aggressive against the gorilla"; but it would seem that some tribes in a

much more primitive state have no scruple in killing them. He has collected a wealth of details about arrow-poison, the various kinds of snares and traps (special attention should be called to the round foot-traps, I 76, 101; II 46, 47) and different makes of arrows. (The crescent-headed types from Angola are curious, and it appears that such arrows, of a larger size, were formerly used in Sweden for shooting foxes. So Commodus may have derived his fancy for decapitating ostriches either from Africa or Northern Europe.) A special feature are the maps showing the distribution of implements and methods of hunting. A list of illustrations would have added greatly to the usefulness of this work; and, if one may venture further criticism on a minor point, the photograph reproduced on p. 70 refers to Nyasaland, not Northern Rhodesia.

A. WERNER.

**Africa, South: Linguistics. Jacottet.**

*A Grammar of the Sesuto Language.*

By the late E. Jacottet. Edited by **47**  
Dr. C. M. Doke. Johannesburg: University of the Witwatersrand Press, 1927. Pp. xxiv, 209. Price 5s.

M. Jacottet, well known to students of African linguistics as the author of a very serviceable "Practical Guide to learn Sesuto," has in the present work given us what, despite certain deficiencies, must be regarded as the most thorough analytical study that has yet been made of this interesting and important language.

The introductory pages of the book provide a sketch of the relation of Sesuto to the other Bantu languages of South Africa, a useful critical survey of previous work on the language and of literature in the vernacular, and brief notes on orthography and dialects. Then follows (pp. 1-17) a short account of the phonetics of the language. This is certainly the least satisfactory part of the work. No attempt has been made to explain Sesuto phonetics on a scientific basis, while the descriptions of some of the sounds, such as, *e.g.*, the "clicks" and the lateral plosives, are far too slight to be of any real service. To say that "the clicks are made by withdrawing the tongue suddenly" is tantamount to saying nothing. The remarks on intonation are interesting, but also very incomplete; in particular the author has completely passed over the significance of semantic tones, which play so important a part in the grammatical processes of the language.

Part II (pp. 18-140), which is devoted to an exhaustive study of the morphology of the language, is much more reliable and important. It is especially pleasing to find that no attempt has been made to adapt

Sesuto to the grammatical scheme of Indo-European languages, a feature which is still only too frequently met with in descriptions of members of other language families. Instead we are given a painstaking account of the grammatical categories found in Sesuto, and are thus able to get an immediate insight into the characteristic structure of the language. All the parts of speech are analysed in great detail, and full justice is done to the intricacies of the pronouns and the verb, the treatment of the latter being particularly elaborate; while the wealth of examples used to illustrate every point dealt with are both illuminating and valuable. It is to be regretted, however, that more prominence has not been given to the linguistic distinctions of sex by means of suffixes, as in this respect Sesuto presents an interesting variation from the typical Bantu scheme.

The final part (pp. 141-209) deals with syntax, and in the extensive treatment accorded this will perhaps be found the most distinctive contribution of the book to our knowledge of the grammatical structure of the language. The studies of word order and of the structure of the sentence are careful and detailed, and should commend themselves to those whose interest lies chiefly in the psychological aspects of linguistic usage.

A word of thanks is due to Dr. Doke for the obvious care with which he has prepared the manuscript for publication. His footnotes are of great value, and serve in many instances to correct or supplement points over which the author passed too lightly.

I. SCHAPERLA.

**Indonesia: Ethnography.**

Clifton.

*Islands of Queen Wilhelmina.* By **48**  
Violet Clifton, of Lytham, with an introduction by Lord Dunsany. Constable & Co., 1927. 284 pp. Illustrated. Price 18s.

In this book Mrs. Clifton, of Lytham, has written an entertaining account of two voyages, one undertaken in 1911 to Java, Sumatra and the small islands lying to the west of the latter, and the other to Celebes. Her narrative is lively and her judgment penetrating. Although the book contains no new facts for the anthropologist yet her keen sympathy and insight into Dutch colonial and native life illustrate in their proper settings customs of which a more detailed account may well be sought elsewhere. Cannibalism, head-hunting, matriarchy—all come into her purview. Her account of Bali and its people does much to reveal the charm which has captured previous visitors.

E. N. F.



Oceania: Religion.

Moss.

*Life after Death in Oceania.* By Rosalind Moss. Oxford Univ. Press, 1925. Pp. xii, 247. 17s. net.

49

The eschatology of primitive peoples is always a subject of interest, and investigation therein is of value by increasing our knowledge of native modes of thought and behaviour. In "The Life after Death in Oceania," Miss Moss has given us a study of mortuary ritual and belief in the after-life among the peoples of Indonesia and the Polynesian and Melanesian islands. The region selected is so wide that only a comprehensive survey has been possible, but, as Dr. Marett observes in the foreword, there is an intimate geographical and historical connection between Indonesia and the many isles of the Pacific which justifies such a choice.

The book gives a valuable summary of the varied burial customs of the region, and, as its main theme, traces the inter-relation between these and the ideas of the natives regarding the existence of the soul after death, and the location of its future abode. But Miss Moss, working with caution, has not subordinated her material to this line of inquiry, and has placed due emphasis on the influence of topographical and cultural conditions in determining the method of disposing of the body of the dead. Cremation, tree-exposure, cave-burial and some forms of desiccation are recognised as being mainly the outcome of natural circumstances, or of practical causes unconnected with beliefs in the fate of the soul.

But on the psychological side the treatment is somewhat inadequate. It has only been half recognised that the mutual influence of ritual and belief, the effect of one concept in producing alteration in another, is the outcome of mental processes, and as such can only be explained, primarily, in psychological terms. For example, it is necessary to demonstrate how such a reaction as "the modification of the idea" of an ancestral home into a belief in "sea ghosts" has actually occurred in the mind of the Solomon Islander, in order to assert convincingly that such has been the case. So, too, the facile phrases, "fusion" of two ideas, "grafted on to an earlier belief," tell us in reality nothing, unless they are based upon actual psychological data from primitive mentality. And as this is lacking, conclusions erected upon these ideas of "fusion" and the like must be regarded as mere hypotheses, however feasible they may appear.

Apart from this methodological defect there are few minor points to note. The term *Reigna* (p. 47) as an alternative to *Reinga* is incorrect. The true Maori

sound is represented by *ng*. The suggestion that there is in Polynesia a definite relation between the idea of an underworld and volcanic activity is, in reality, not supported by any native belief; while the idea that widows in New Zealand were *killed* on the death of their husbands is also a misconception. It is doubtful whether their suicide can be really termed "a trace of the Melanesian form of human sacrifice." The idea of the Maori after-world as a "fusion" of Melanesian and Polynesian beliefs needs support from much stronger evidence in order to carry conviction.

But allowing for these details, however, the book offers a valuable and suggestive treatment of the burial forms and eschatological beliefs which characterise the region.

R. W. F.

Britain: Archæology.

Moir

*The Antiquity of Man in East Anglia.* By J. Reid Moir. Cambridge: At the University Press, 1927. Pp. 165. Price 15s.

50

The story of man's evolution as outlined in this volume provides much food for thought, and since this is the first occasion upon which the views of the progressive school of archæology have been fully published in book form, "The Antiquity of Man in East Anglia" is of paramount importance to all students of prehistory who wish to appreciate the true significance of the many archæological discoveries made in East Anglia, where geological circumstances provide so unique an opportunity for accurately reconstructing Man's past history. The author has employed as a foundation for his own work the achievements of Frere, Collyer, Skertchley and Harrison. These have been followed up and amplified with singular success, with the result that Mr. Moir has succeeded in building a very convincing reconstruction of life in prehistoric times in this country; and if all the claims which have resulted from his activities as a field archæologist represent facts, they would best be summarized in the words "Si monumentum requiris circumspice." The material which composes the majority of the chapters should, strictly speaking, be considered controversial—that is to say, there are people who oppose the interpretations the author has placed upon certain of the discoveries he has made. But whereas it will be found extremely hard to discover a weak link in the reasoned chain of argument which holds these particular chapters together, it should be noted that Mr. Moir's opponents have, up to the present, failed to produce a single piece of

constructive evidence in support of their oft-repeated denials of the human origin of the various implements in question. The Harrison eoliths; the sub-Crag and Crag specimens; the two industries located, *in situ*, at the base of the Cromer Forest Bed Series; the flakes and implements from the Lower Glacial Clays, the Inter-Glacial Gravels, and the Upper Glacial Clays; and the artifacts from certain of the occupation floors situated in Messrs. Bolton & Co.'s brickfield, near Ipswich, have, one and all, been claimed by this opposing school of thought as the results of the unbridled forces of Nature. Were these views correct it would follow that the greater portion of the author's labours must prove void. Fortunately for him this is not so. Quite apart from any other considerations it should be recognised that all the specimens discovered by Mr. Moir and claimed by him to represent the work of Man conform with the requirements demanded by universally accepted implements. Further, with the possible exception of floors A to D, situated

in Messrs. Bolton & Co.'s brickfield, the geological age of the deposits in which the various specimens have been found cannot possibly be disputed. The observations published by Dean Buckland after his investigations in Paviland Cave make strange reading when compared with those of Professor Sollas in his Huxley Memorial Lecture of 1912. But it must not be forgotten that practically a century separated these pronouncements, and that Professor Sollas had the advantage of the increase of knowledge which had resulted since the commencement of the nineteenth century. The views expressed by Mr. Moir's opponents when compared with the contents of the volume under review make even stranger reading, and it is hard to regard them both as being contemporary. If only to justify their existence the author's opponents should hasten to publish a restatement of their case before it is too late. The ground is fast being cut from under their feet as "The Antiquity of Man in East Anglia" clearly proves. J.P.T.B.

### CORRESPONDENCE.

Sardinia.

#### The Cart-ruts of Malta.

To the Editor of MAN.

SIR,—The cart-ruts of Malta described by Miss M. A. Murray (MAN, 1928) remind me that, apart from Italy and the South of France, Sardinia also provides examples; one notable example occurring on the steep hill immediately north of Cagliari, the southern capital. This hill bears a ruined Pisan castle named Castello San Michele, and rises abruptly out of the plain to nearly four hundred feet above sea-level. It is a bare outcrop of hard sandstone. Across the brow of the hill and close to the castle are two parallel ruts scored some six inches deep in the exposed rock and running underneath (and therefore older than) the Pisan work. These ruts are similar to the *trieks* of Malta. I have examined both Maltese and Sar-

Suffern.

dinian ruts. The Maltese ruts have been described many times and there are excellent illustrations of them in the Westons' "Camera Pictures of Malta." Similar ruts, but not so deep nor so straight, are made by modern carts on the surface of bare rock. To my mind it seems obvious that these ruts were made by the frequent passage of wheeled vehicles over bare areas of rock, an easy passage being procured by keeping to the track of predecessors. This explains why there is not a multiplicity of tracks close together and proceeding in the same direction. The wheels were in all probability solid stone discs such as can be seen in parts of Sardinia to this day. Where a country road runs over a shelf of outcropping rock in Sardinia there is to be found a single pair of ruts, not several.

Yours faithfully,

C. SUFFERN.

### ANTHROPOLOGICAL NOTES.

*Expedition of the Royal Anthropological Institute to the Fayum.*—The following appeal on behalf of Miss Caton-Thompson's Expedition to the Fayum has been made to Fellows and others by the Council of the Institute, and has produced about £95 up to date, but it is hoped that even more substantial aid will be forthcoming for this important work, which urgently calls for the support of all who

are interested in the early history of Man in ancient Egypt:—

ROYAL ANTHROPOLOGICAL INSTITUTE.  
52, Upper Bedford Place,  
London, W.C.1.

February 23, 1928.

DEAR SIR or MADAM,—Last autumn the Institute obtained from the Egyptian Government a concession to enable Miss Caton-Thompson to continue her work of

excavation in the Fayum, Egypt. When the Institute, at Miss Caton-Thompson's request, applied for this concession, it was clearly understood that it could not be financially responsible for the expedition. The Council, however, promised to assist Miss Caton-Thompson as far as possible in raising funds towards expenses. The cost of the expedition was estimated at £1,000 at least. It was hoped to obtain considerable assistance from America; but the application was not successful.

The funds with which the expedition started are now practically exhausted, and, unless further subscriptions are received, Miss Caton-Thompson will have to return before completing the work planned for the season.

Up to the present Miss Caton-Thompson has found relics of the IVth Dynasty and the remains of a Ptolemaic irrigation system. The latter has aroused the personal interest of the Egyptian Minister of the Public Works Department, and for this part of the work he has now supplied some labour free of cost. Miss Caton-Thompson's main objective, however, is to find evidence for the date of the early civilization of which she has found remains on her two previous expeditions. In this she has not yet been successful. It would, therefore, be extremely unfortunate if her work had to close down before the end of the season, while there is still a possibility of success. I have, therefore, been asked by the Council of the Institute to invite Fellows and others to subscribe a sum sufficient to keep the work going for the remainder of the season. About £500, at least, is required, towards which Dr. Rushton Parker has generously offered to give 10 per cent. of any amount up to £1,000 which may be collected. Subscriptions should be made payable to the Treasurer of the Institute and sent to me at the above address.

Yours faithfully,

E. N. FALLAIZE,

Hon. Sec.

*Edinburgh Branch of the Royal Anthropological Institute.*—Professor V. Gordon Childe on "The Horse's Bit":—

Professor V. Gordon Childe, of the University of Edinburgh, delivered his first public lecture on 13th December, at a meeting of the Edinburgh and Lothians Branch of the Royal Anthropological Institute. His subject was "The Early History of the Horse's Bit." He said that the peculiarities of ancient harness would tend to make the horse hard-mouthed, and hence some special provision had to be made for controlling the animal. The curb chain being unknown, the ancients

relied partly on lateral pressure on the jaws, effected by two cheek-pieces at either end of the jointed bar that constituted the bit proper. From a study of these cheek-pieces two main families of bits could be identified. On Egyptian and Mycenaean monuments of the fifteenth and fourteenth centuries B.C. a type with flat metal cheek-pieces is depicted. Though not even in Tutankhamen's tomb have actual bits survived ancient plunderings, those depicted clearly agree with rather later specimens from Egypt and Mycenae, which have spikes on the inside of the cheek-pieces. The same type is very distinctly represented on Assyrian monuments of the ninth century B.C. But after 720 B.C. a new type appears there, whose origin is traceable to horn or antler cheek-pieces, such as have been found especially in the Swiss lake-dwellings.

The lecturer's recent excavations in Hungary had shown, he said, that the type went back at least to the sixteenth century there; the bit proper was probably a twisted leather thong. This horn and leather bit was translated into bronze about 1100 B.C. About the same time horn cheek-pieces and bronze bits of the same family were found in graves of the North Caucasus region, together with Hungarian types of ornament. Hence the introduction of the "European" type of bit into Assyria might be connected with the incursion of the Cimmerians, who were, according to Herodotus, driven from South Russia by the Scyths, and are first mentioned in the Assyrian records about 720 B.C.

*The Tuareg.*—Mr. Francis Rodd published in *The Times* of 19th and 20th March, a preliminary account of the bearing of certain petroglyphs at Air in the Sahara on the question of the racial origin of the Veiled Tuareg. One difficulty in the way of the acceptance of his identification of Libyan captives in Ancient Egyptian pictures and records as the ancestors of the Tuareg had been the absence of the veil: they wear a headdress composed of ostrich feathers. The petroglyphs at Air wear what appears to be exactly the same headdress. It was suggested that the Tuareg occupied Air and adopted the veil at some time between 600 and 1000 A.D. The association of the Tuareg script with the petroglyphs indicates that they are the work of the same people, not of the preceding negroid population. Thus, as late as 1000 A.D., the Tuareg were producing pictures of themselves identical with those painted by the Ancient Egyptians in the XIXth Dynasty of the people west of the Nile.



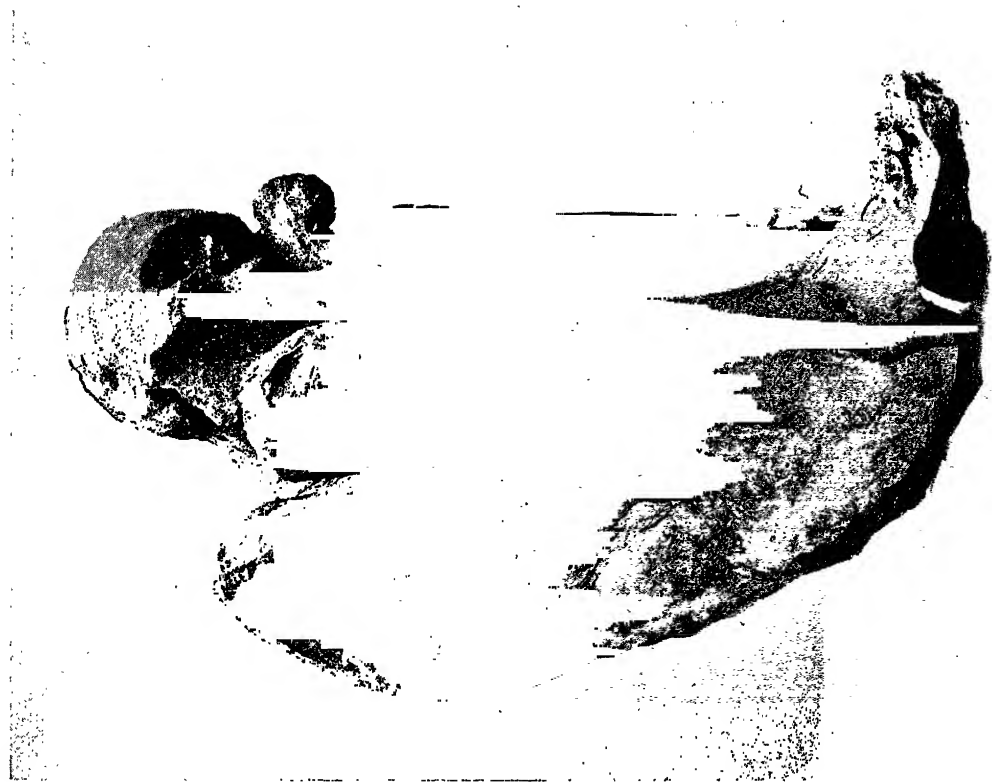


FIG. 2.



FIG. 1.

TWO MUMMIES FROM COLOMBIA.

## ORIGINAL ARTICLES.

With Plate E.

Colombia: Mummification.

Dawson.

**Two Mummies from Colombia.** By Warren R. Dawson. With Plate E.

53

I am about to publish, in the *Journal* of the Institute, a somewhat detailed essay on mummification in America, and as some interesting specimens have come to my notice since the manuscript was completed, it will be convenient to publish descriptions of them separately. I now offer descriptions of two of these specimens, preserved in the British Museum, for the examination of which I am indebted to the facilities kindly afforded me by Mr. H. J. Braunscholtz.

I. Brit. Mus. No. 38-12-1-1. The original label reads as follows: "Desiccated body found in a cave near Leiva, N. Granada. Presented by W. Turner, Esq., H.M. Envoy to Colombia." (Plate E, Fig. 1.)

The mummy is that of an adult female in a sitting position, the arms being placed, with the forearms almost parallel, across the chest, the right over the left. The left hand is clenched, the right has the tips of the fingers bent inwards. The head is bent slightly forwards and inclined to left. Knees vertical. Left foot crossed over right. Toes of right foot bent upwards, those of left downwards. All toe-nails missing, and apparently finger-nails also, but, owing to the position of the hands, this cannot be ascertained with certainty. Impressions of woven cloth visible on all prominences, showing that the body was formerly wrapped up. Scalp missing from the left side of the top of the head, exposing part of the sagittal suture and the lambda. The condition of these sutures suggests middle-age. Top of head apparently bald, but elsewhere long black straight hair (20-30 cm.) hanging over shoulders. Traces of dark red pigment on the bare patch of the skull (? blood-stains). At the base of the right scapula the integuments are missing, exposing the end of the scapula and part of three ribs. Round the edges of this wound a bluish-black pigment has been applied.

Round the neck, a necklace (apparently restrung on modern string) of thirteen objects, ten of which are celt-shaped pieces made of shell, each about 40 mm. long, and three canine teeth of a carnivorous animal, pierced for suspension at the root-end.

There are traces of a reddish pigment on the thighs, knees, cheeks and elsewhere, which may be derived from a red-dyed wrapping. On the cheeks also (which are marked with the impression of cloth) there is a patch of reddish colour.

The skull is very high and broad in proportion to its horizontal axis. The ear-lobes are not pierced, and the left auditory meatus is plugged with a large pad of wool. Mouth open: nearly all the upper incisors lost; lower teeth all present, except two middle incisors. Teeth much worn, exposing the pulp cavities. Mouth packed with wool, which has distended the cheeks. No trace of tongue. The nose is broken away, and most of the nasal cartilage is missing, but the septum is intact. There is no forced passage into the cranial cavity through the nose.

The perineum has been incised, and the anus and vulva united into a single opening and widely distended. This was probably done for evisceration, but, owing to the near proximity of the heels, it is impossible to make a close examination. The opening is full of desiccated scraps of tissue, probably the remains of the viscera that had been dragged out through it. There is no trace of pubic hair.

The general state of preservation is good, and the condition of the mummy suggests that it was probably smoke-dried.

II. Brit. Mus. (no number. ? 1842), labelled: "Dried body with its cerements. "Gachansipa, N. Granada. Presented by R. Bunch, Esq., of H.M. Legation." (Plate E, Fig. 2.)

Mummy of an adult male, in poor condition. Contracted posture, with head inclined forward and resting on right hand above left knee. Right arm inside right leg, bent at elbow, with hand and clenched fingers above left knee. Left hand also clenched, and placed behind left knee under the left ear.

The integuments of the back and of the right leg and foot are fairly complete, but those of the left leg and foot have largely flaked away. Wherever the integuments remain, they are covered with a deep red stain, almost black in places, and resembling a resinous application. The anterior part of the abdominal body-wall is broken away and missing, exposing an entirely empty cavity. As the thorax (which is not broken) is empty and contains no remains of the thoracic viscera, it seems almost certain that the body has been eviscerated. On a close examination it would appear that an incision has been made in the left flank, and that through this, all the internal organs have been extracted.

Most of the scalp has disappeared, and what remains is in bad condition. The eyes have collapsed into the orbits, but the eyelids apparently were not closed and have retracted. The right external ear is missing. The mouth is open, exposing the much-worn teeth. The oral cavity is empty and contains no packing material. The anus is large and gaping. The penis and scrotum are flattened behind the right heel which is pressed against them. The glans has receded far into the prepuce, which has a sharply cut edge and suggests that circumcision has been practised. The retraction of the glans is doubtless due to the inward pulling of the muscles when the viscera were extracted. There is no trace of pubic hair.

The condition of this mummy, like the last, suggests that it has been smoke-dried.

The present note is confined to the actual description of the specimens. Their significance will be discussed in my forthcoming paper.

WARREN R. DAWSON.

## Ireland: Archæology.

Whelan.

**Ireland in Pleistocene Times.** By C. Blake Whelan, M.A., B.L.

54

[Mr. J. P. T. Burchell announced in *Nature*, 20th August, 1927, that he had discovered a series of Early Mousterian implements made of limestone in a collapsed rock-shelter at Rosses Point, Sligo, Ireland, geological confirmation being supplied by his finding of similar implements, *in situ*, near the base of a deposit of boulder-clay in the same area.]

These announcements met with considerable opposition from a certain section of Irish archæologists, who have stated that Mr. Burchell's artifacts are nothing more than angular beach material resulting from wave-action. (*Nature*, 5th November and 31st December, 1927.)

Mr. Hazzledine Warren has also attacked these same specimens. (MAN, 1928, 4.)

On the other hand there is a powerful group of archæologists which has pronounced very definitely in favour of a human origin for the Sligo material. (*Nature*, 28th January, 1928.)

The following article, therefore, contributed by Mr. C. Blake Whelan, who has visited the sites in question, cannot fail to be of interest to students of prehistory who are following this controversy.—EDITOR.]

Professor J. K. Charlesworth has mentioned in his note on Palæolithic Man in Ireland\* certain obstinate facts which he considers to persist in this particular connection. He emphasises especially two time-honoured traditions: first, that during the period of the Mindel glaciation the ice-sheet in England attained its maximum southerly extent at the line of the Thames and the Bristol Channel; and, second, that no trace of Palæolithic Man has ever been found north of the

\* *The Irish Naturalists' Journal*, November, 1927, pp. 278-9.

York-Shrewsbury morainic line to which the ice had withdrawn at the period of the Riss. He describes in vivid language the conditions which he assumes to have followed, in Great Britain and Ireland, upon these great ice movements. It may reasonably be gathered from Professor Charlesworth's account that throughout the vast inter-glacial period which succeeded the extremes of the second glaciation in N.W. Europe, Palæolithic Man in England dwelt far to the south in the midst of semi-polar climatic conditions and a tundra fauna. In Ireland, even during the so-called third interglacial when it is generally agreed that the Mousterian industry had reached its most characteristic phase elsewhere, Professor Charlesworth pronounces against Palæolithic Man a "ne plus ultra" at the Shannon-Wexford line. This very significant paper concludes with an eagerly-awaited allusion to the reported discovery by Mr. J. P. T. Burchell of Mousterian implements along the low carboniferous limestone cliffs bordering portion of the coast-line of Co. Sligo. Professor Charlesworth tells us that, in the company of Professor Macalister, Dr. Praeger and Mr. Stelfox, he most carefully examined the Sligo sites and implements, but failed to find any evidence whatever of the existence of Palæolithic Man in Sligo. In his opinion, the shelters in question are merely sea-caves, probably not 100 years old, while the implements are merely angular beach material. It should be noted that this latter opinion was hazarded without even submitting to examination the material which Mr. Burchell had collected (*Nature*, 31st December, 1927). These uncompromising statements from an authority of such geological eminence are of the greatest interest to many who are less inclined than Professor Charlesworth to adopt the traditional position of scepticism in regard to Mr. Burchell's altogether remarkable type series of limestone flakings in early Mousterian forms, some of which are now exhibited in the British Museum. Others beside the delegation of the Royal Irish Academy have investigated, on the spot, the problem of the Sligo sites and implements and have, indeed, been more fortunate than the delegation, on its earlier visit, in finding both. It is certainly true that the question which Professor Macalister has very properly raised as to the geological nature of the provenance requires the most exhaustive examination. Few, however, who have studied what has already been written on the subject will have failed to realise that the solution of the rock-shelter problem will entail a longer view and more subtle argument than a theory of recent sea-caves and angular beach material. As to the latter, Professor Charlesworth will doubtless agree that the opinion of certain experienced students of the Mousterian culture who have pronounced in favour of the Sligo specimens as being artifacts cannot lightly be discounted. Regarding the attribution of the shelters to present-day erosion of the sea, there are, among many, the following considerations: the conformation of the land at the glaciation period, extending as it then did to the Continental Shelf; the obvious probability of caves and shelters of high antiquity in this, as in other, limestone areas; the possibility of preservation in position of the fallen fragments of a shelter roof beneath a depth of boulder clay and their eventual exposure by an encroaching sea.

Time is needed for much further work both below and in the Irish boulder-clays and inter-glacial gravels; but in the meantime one may, perhaps, be permitted to ask one or two questions which seem to have some bearing on Professor Charlesworth's postulations. First, as to the hypothetical line from the mouth of the Thames to the Bristol Channel, which is still so firmly held by certain geologists as the southern extremity of English glaciation. One would have thought that the discovery two years ago of large masses of boulder-clay, containing Mousterian implements, as far south as Hastings, would have enforced a general withdrawal from this ancient and hard-fought position. Secondly, there is the well-worn axiom that Palæolithic Man has not been brought to light, and is, therefore, not



to be found, north of that other archæological entrenchment between Shrewsbury and York. One would like to know whether Professor Charlesworth is aware of the establishment in 1926 by Mr. J. A. Cree of the existence of man in Scotland in the late Palæolithic period.\*

As is well known, it had hitherto been considered that the earliest evidence of man's inhabitation of Scotland dated from Azilian-Tardenoisian times.

There is also Professor Charlesworth's graphic pen-picture of the tundra which seems, in his view, to have been the only inhabitable *terrain* in these islands during tremendous stretches of interglacial time, a very limited area of ice-bound waste. Making the utmost allowance for the adaptability of the African-Asiatic mammals, it is still not a little difficult to reconcile such a view with the evidences of a flourishing Chellean culture at Gray's Thurrock, Ilford, Essex, and along the Thames Valley generally, in association with a warmth-loving fauna. It is also more than a little difficult, in face of the evidence of both fauna and flora during the English Lower Palæolithic period, to accept as flawless Professor Charlesworth's contention that Palæolithic Man need not be looked for in Ireland north of the Shannon-Wexford line. It should be remembered that significant discoveries have been made in past years in Co. Sligo. The Caves of Keshcorran,† situated some twenty odd miles to the south-east of Mr. Burchell's sites, were found to contain the remains of the Reindeer and the Arctic Lemming. The excavators were prevented, in places, from reaching the rock floor of these caves owing to the occurrence of a deposit of boulder-clay, the removal of which would have entailed too much labour. It may reasonably be presumed that the same deposition of boulder-clay which overlay the basal deposits in the caves of Keshcorran, also smothered-up the rock-shelter site at Rosses Point. Certainly it is evident that caves existed in Co. Sligo during glacial times, and the next question for determination follows inevitably upon this conclusion. Who and what occupied these caves during the interglacial periods of Pleistocene glaciation? Mr. Burchell's discoveries of last year would seem to supply an indication.

C. BLAKE WHELAN.

## Africa, East: Religion.

Huntingford.

**Ghosts and Devils in East Africa.** By G. W. B. Huntingford.

I. BANTU.

55

1. *Īsukha* (Kakamega of North Kavirondo, Kenya Colony) *Shinanyentsu* (pl. *vinanyentsu*). The spirits of the dead visit the huts of sick people; they are not seen by those in health. When a ghost appeared, it was formerly the custom to go to a medicine-man (*umukhōmu*), who made medicine (*amasambu*) to exorcise the ghost. A goat was then killed, and some of its blood mixed with the medicine was put into a pot, and the goat and the pot were placed on the ground at some distance from the hut for the ghost to eat. This custom seems to have fallen into disuse owing to missionary influence, and the *Īsukha*, instead of attempting to placate the ghost, merely says, *Intse ne muluali, nakhutsa, ka 'makhōba ka Liuva* ("If I am ill and die, it is God's will").

2. *Īsukha*: *Linani*. Formerly a big black dog went about by night, with sparks coming from its mouth, looking for people to devour. By day it slept in the grass. It was called *Linani*, and has now disappeared.

3. *Nyala* (*Kabaras* of North Kavirondo): *Shinanyentsu*. The spirits of the dead (*shinanyentsu*) live in holes in the ground. People in good health do not see

\* *The Scotsman*, 15th February, 1927; *Antiquaries Journal*, October, 1927, pp. 523-4; *Proc. Soc. Antiq. Scot.*, 1926-7; Vol. LXI, pp. 169-72.

† "The Caves of Keshcorran," *Trans. Roy. Irish Acad.*, Vol. XXVI, pp. 189 *et seq.*

them; but when a person is very ill, they are liable to come into his hut in great numbers, taking the form of white lizards (like the house-lizard, *livakala*), and, getting all over the sick person, they "tread on him" (*wamusenaka 'muntu*) and bite him, and finally he dies.

4. *Nyala*: *Umusambwa kwa Wutukhu*. There is said to be a kind of evil spirit which appears at night and takes the form of a big black bird. On account of this evil creature, people do not like to be out alone at night, nor even to be alone in a hut, for the walls of a hut are no barrier to these spirits. One of my boys, a *Nyala*, one day came up from his work before the proper time, and complained of feeling ill. Thinking he might have some infectious disease, I told him he was to spend the night alone in an empty hut. Just before dark I sent a boy to him with some food; he came back and said that *Lukamasya* (the sick boy) was "going to die of terror." I went up to him, and asked him what the matter was, and he told me that if he was alone in that hut at night the evil spirits would come at midnight, flapping their wings, and carry him off. Seeing that he was frightened out of his wits, and fearing serious consequences if he was left alone, I let him go to his own hut. Next morning he was completely recovered.

5. *Syan* (N.E. of Bugishu in Uganda): *Omusambwa*. It is customary among the *Syan* to make yearly offerings of eleusine corn (*ekiru*) to the spirits of the dead (*emisambwa*). Should a person neglect this offering, he is liable to be visited by the angry spirit, which comes at night into his hut (whether the door is open or not), and seizes him by the neck with its hands. The person wakes up and feels pressure on his neck: in the morning he is ill, and unable to eat. He then knows that he has been visited by a ghost of an ancestor. He must go without delay to a medicine-man (*omufumo*), who tells him what to do. The usual procedure is to offer a goat or fowl, and some beer (*amarua*). The goat or fowl is killed and put in a tree at some distance from the sick man's hut; the beer-pot is put in a hole dug for it just outside the door of the hut. If the spirit accepts these offerings, the man will recover; if it does not, he will die.<sup>(1)</sup>

6. *Kusu* (*Kitosh* of North Kavirondo, Kenya): Lycanthropy. Certain witch-doctors of this tribe are said to turn into hyænas (*namunyu*) at night, and to prowl round the huts of people they dislike, uttering the hyæna's howl. I heard of a *Kusu* witch-doctor on a farm near Turbo Valley (Kenya) who, so the boys said, did this. But no one has ever seen a were-hyæna; every one is far too terrified to look at night when a hyæna is howling close at hand: and it is probable that the cases of so-called were-hyænas are really exhibitions of ventriloquism.

## II. NILO-HAMITIC.

7. *Nandi*: *Chemosit*. There is a devil with one leg, nine buttocks, and a mouth which shines like a lamp, whose name is *Chemosit*. It wanders about at night, looking for children to eat, whom it entices by singing.<sup>(2)</sup> I suggest that some perfectly harmless animal may have been the origin of this devil. I was once going through North Nandi in the evening in a car, with two other people. We saw in a tree what looked like a red lamp, which got off the tree and went away, apparently hopping, through the grass and across the road in front of us. What we saw was the eye of some small creature, probably a squirrel, and, as its body was invisible, the up and down motion of the eye as it moved gave the impression of something with one leg hopping.

8. *Nandi*: *Tobôhwa Hill*. This hill, in N.W. Nandi, is said by some Nandi to be haunted; they say that the spirits of people, cattle and goats may be heard there. Other Nandi (e.g., *Arap Kipsambui*, of Masop, one of the folklore authorities) deny this. It is evident, however, that the people of the district do not like the hill, which is rather inaccessible, being nearly surrounded by pathless forest.

I have been there twice, and on neither occasion did I see huts anywhere near, not did I even meet people near it.

9. *Nandi*: *Chemosisiek* at Kipkorom, near Tindiret. When visiting the Dorōbo at Kipkorom in the Tindiret district a few months ago, in the company of the Hon. P. L. O'Brien, M.A., we were much puzzled by a wailing noise which came from the forest at night. The noise resembled the whistle of an Uganda Railway locomotive, or a (Nandi) wooden pipe. It cannot have been either of these, however, because (a) the railway is far too distant to hear an engine; (b) it went on all night, and we heard it for half an hour at a stretch. The Dorōbo said they knew nothing about it; a Nandi who had lived in that region when a child told me that it was the devils (*chemosisiek*).

10. *Māsae*: *Il-Meneñgai* or *I-Meneñgai*. "The Masai say there are no such things as ghosts because they do not see them. But it is supposed that cattle see them, and when a herd of cattle all gaze at one spot, they are said to be looking at either a ghost or a beast of prey" (Hollis, "Masai," 308). This rather strange statement (translated from a Māsae original) is at variance with the story about Meneñgai Hill, near Nakuru, in the Il-Muteita country, the name of which means "ghosts" or "corpses," and concerning which the Nandi (who never have lived there) say that the ghosts steal peoples' belongings if they put them on the ground. If the statement quoted by Hollis was made by Justin Ol-omeni of the C.M.S., it can be understood; if not, it can hardly be true.

G. W. B. HUNTINGFORD.

#### NOTES.

(1) Similar beliefs are found among (a) the WaGisu of N.E. Uganda: "a medicine-man sometimes attributes sickness to ghostly possession, which he says must be cured by propitiating the ghost" (Roscoe, "The Bagesu," 39); (b) the Kamechak or Nandi Sabei (Roscoe's "Basabei"): "when a man brewed beer, he always poured out a little for the ghosts of his father and grandfather, for if this attention was neglected they would be offended and bring illness into the family" (*ib.*, 59). Roscoe also quotes a case when a ghost took an offender by the throat (*ib.*, 59); (c) the Bambwa of Ruwenzori: "when the cause of a man's illness had been found by the medicine-man to be a ghost of his own family, the ghost had to be appeased by the offering of a goat" (*ib.*, 153); (d) the Batuse of Ruanda: "if the augury showed the illness to be due to a ghost, a bull was offered to the ghost, and it was begged to stop troubling the man" (*ib.*, 197); (e) the Lango: when people are haunted, "it may transpire that some ceremony has not been adequately performed, and the spirit can then be readily pacified by the correction of the error or omission, and by sacrifice" (Driberg, "The Lango," 232).

(2) With the monstrous East African devils, like the Nandi *Chemosit*, the Māsae *En-e-'n-aurir* ("the-of-the-stake") which "was formerly a lion, but it changed itself, and one-half became a man, while the other half became a stone" (Hollis, "Masai," 265), and the Gala *Adi*, the white devil which could change its shape, we may compare the Greek monsters *Ἐμπουσα* (or *Ὀνοσκελῖς*, *Ὀνοκόλη*, "ass-footed"), the creature sent by Hekate (Ar. Ran., 293); *Λάμια*, who fed on human flesh (Ar. Vesp., 1177); and *Γελλῶ* or *Γελῶ*, on which Zenobius, Parcm., I, 58, says *Γελλῶ γάρ τις ἦν παρθένος. καὶ ἐπειδὴ ἄρως ἐτελεύτησε φασὶν οἱ Λέσβιοι αὐτῆς τὸ φάντασμα ἐπιφοιτᾶν ἐπὶ τὰ παῖδια καὶ τοὺς τῶν ἀρῶν θανάτους αὐτῇ ἀνατίθεασιν*.

Britain: Archæology.

Newton.

**On Deneholes or Drainage Pits and their relation to Grime's Graves, or First Antler-Pick Period.** By W. M. Newton.

56

These wonderful constructions of our prehistoric past were known to the Roman invaders of Britain and have remained an archæological puzzle ever since. The following extract is taken from the section on deneholes in the "Victoria History of the County of Kent," p. 449; the localities mentioned are to the west of Dartford Heath and are not far from Row Hill, which has an altitude of about 300 feet: "Leaving the wilder parts of Kent, we come to Bexley, where unquestionable deneholes are not only more numerous than elsewhere, but

“ where they may best be examined. For, as already mentioned, pits scattered singly here and there have generally been more or less filled up to prevent accidents, if they have not collapsed or been choked up through the influence of the weather during centuries of disuse. But groups of some forty or fifty deneholes concentrated as closely as the separation of each pit from its neighbours allows (like those of Stankey and Cavey Spring, Bexley), have a much better chance of survival, for the small compact area they occupy has obviously been rendered unfit by them for every other purpose, and can only be fenced in and let alone.”

For the purpose of this paper the terms Denehole and Drainage Pit shall be synonymous. The Kentish historian Hasted is, I think, responsible for some misleading remarks as to the nature of deneholes. In Vol. I, p. 24, he writes: “ There are in the heaths and fields and woods many similar pits or artificial caves.” To describe deneholes as occurring in heaths, woods and fields was as true in Hasted’s time as it is now, but such a description would be incorrect at the time when the deneholes were made. They were sunk on the bare Thanet sand for the purpose, I now suggest, of getting rid of troublesome surface water during periods of torrential rains. It may be that two simple wells represent the first efforts in that direction; they are referred to by Mr. W. Whitaker in his “ Geology of London,” 1889, Part I, p. III, “ two dry wells in Joydens Wood :—

“ 1. Thanet sand, 60 feet. Chalk, 43½ feet.

“ 2. Thanet sand, 25½ feet. Chalk, 70½ feet.”

Mr. W. Whitaker also states that in the shaft of a denehole in the north-western corner of this wood 46½ feet of Thanet sand was found above the chalk. This last remark refers to one of the deep deneholes. To sink a truly vertical shaft 60 feet in Thanet sand argues the use of a plumb-line, a line probably made of strips of bullock hide like a lasso. Ropes made with strips of twisted bullock hide need not be very thick to bear the weight of a man; it requires but a thin lasso to bring a bullock down. It may also be imagined that the civil engineers of, say, 5000 B.C. used small hide buckets to convey the material excavated to the top of the shaft; buckets would be filled by hand, large blocks of chalk having been broken into small pieces. It is possible, as is the present custom in the East, that a small army of youthful workers may have been engaged in carrying the buckets full of chalk or sand to the dumps. The quantity of sand and chalk taken out of each deep denehole would form a large mound. No traces anywhere of such mounds exist to-day. The deep deneholes bear a family likeness—the long vertical shaft sunk through Thanet sand and continued for a few feet in chalk—the large chamber in the chalk with its extensive floor space—supports left to roof as in mines, and a cone of sand standing on the floor of the chalk chamber immediately under the shaft.

In cases where the situation has not permitted an extensive floor, apses were constructed in the wall of a small chalk chamber. Mr. F. W. Scudder, of Belvedere, who some years ago explored three deneholes of the group excavated on sloping ground at Lesney Heath Farm, found six apses in each denehole, three on one side of the chamber and three on the opposite side. A cone of sand, nearly closing up the bottom of the shaft, stood upon the floor of each denehole. It may be assumed that all the drainage pits in this group were constructed alike.

Many deneholes have remained open to the sky; some have had a layer of clay over the mouth of the shaft, thus preserving the sand cones clean and free from any rubbish that might have fallen down the shaft. An example of this kind was discovered on 13th January, 1920, in the garden of a house occupied by Mr. W. J. Challis, 23, Bath Road, Dartford. Mr. Challis assured me that, in order to attend to some poultry, it was necessary during three years to pass over,

and occasionally to stand upon the ground, immediately over the shaft of this denehole. The falling in of the ground, it is believed, was caused in the first instance at the time of making the poultry run, when a post, as was afterwards discovered, had been driven in the ground just touching the edge of the shaft. There was a drop of 18 feet to the apex of the sand cone, and although this cone filled up a large part of the chalk chamber, it required 187 cartloads of earth to fill up the hole at the time of discovery and 20 additional loads since then to make up for settlement. Unknown deneholes, and many that have been filled in, are an ever-present danger in this district. A very fine, deep denehole was broken into sideways a few years ago at Stone, near Dartford, by men quarrying chalk for cement-making. Another denehole was discovered in a most curious way at Gravesend on a southern slope of Windmill Hill, at an altitude of 75 feet above O.D., the top of the hill being 196 feet above O.D. The following account is given by Mr. Alex. J. Philip in *The Home Counties Magazine* for September, 1909 :

" Its discovery was entirely accidental. The land had been opened up for building, and workmen were engaged in sinking a cesspool. When some 50 feet below the surface, the supposed solid chalk fell away from the feet of a workman, who, very much to his surprise, found himself precipitated into this underground cave. Fortunately, he was uninjured, although considerably startled." Mr. Philip has named this drainage pit the Twin Chamber Denehole, and found in it clear marks of a deer-horn pick.

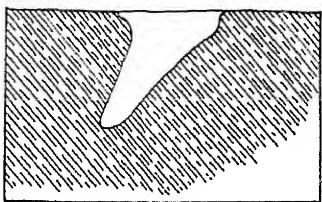


FIG. 1.—DIAGRAM SHOWING PICK-MARKS ON THE WALL OF THE GRAVES AND TWIN-CHAMBER DENEHOLE.

(By kind permission of Mr. Philip.)

" On the roof of the smaller chamber, not quite so lofty as the other, was a curious smoothness of the chalk surface." The cesspool had been sunk through clay, Thanet sand and chalk parallel with the shaft of the denehole, with only about 6 feet between. If this unique form of denehole was really designed to trap a flow of water coming from higher ground, it apparently could not have been better placed, for it has functioned admirably; the " Old " Dover Road, probably then, as now, passed by it at a lower level. Flowing from the top of the hill over the Woolwich beds and Blackheath pebble bed, the descending water became charged with particles of sand. The two chambers of the Gravesend denehole have a total superficial floor space of about 1,200 square feet, upon which sand rests to the extent of about 9 feet deep, thus giving an approximate measurement of 10,800 cubic feet, to which must be added the bulk of the enormous cone of sand that stands under the long-closed shaft.

The group of drainage pits previously mentioned, at Lesney Heath Farm, were placed a little away below the brow of a gentle slope, but the Gravesend pit was over a 100 feet below the summit of a hill. The writer visited this Twin Chamber Denehole soon after its discovery, and tramped about in the soft floor of sand which may have required many seasons of heavy rain to accumulate; and after that, owing to a great diminution in the volume of water, the height of the cone increased slowly until, by degrees, the action of the water lessened and, finally, the last drops carried down the last grains of sand. Afterwards came a sheet of clay which thrust itself over the mouth of the shaft, closing it, probably, for ever. The land about has been built over, and the fine old denehole is again in use as a drainage pit. The sheet of clay, several feet thick, was described by a friend as " bull head " clay.

I must not omit to mention that a wall of chalk divided the twin chambers of the Gravesend denehole; that a 6 feet square aperture was cut through this wall at the floor level; also that, at the top, the wall had been cut away from the

roof, thus leaving a clear space of some inches across its entire width between the top of the wall and the roof. Through this slit, water, with sand in suspension, passing from the more lofty chamber, would most likely scour the roof of the lower chamber, and so produce that smoothness recorded by Mr. Philip.

When, in the year 1878, I came to live at Dartford, I found that my house was near to a denehole that had a local legendary reputation as a secret underground way to the old Priory, half a mile away in the valley. Mr. E. C. Youens, Curator of our local Museum, has kindly given me a memorandum made by himself, describing a descent made by his elder brother, Jesse, into this hole: "On the south side of Shepherds Lane a denehole or ancient pit, known as the "Sound Hole, the shaft of which had evidently been considerably widened by "the rain-wash of centuries. It was explored by Jesse Youens in September, "1873, by the aid of a rope which was fastened to the branch of an overhanging "tree. He slipped down this rope and alighted on a steep slope of earth that "descended into three caves branching off; they appeared to extend only a short "distance. The hole was about 30 feet deep." This discovery revealed the existence of a simple drainage pit with three apses or soakaways, cut laterally in the chalk, to facilitate the absorption of incoming water. In this aspect the deneholes of Lesney Farm Wood, with their twice three apses or soakaways, may also be appreciated as drainage pits. The deep deneholes will also come into the same useful category with their greatly extended floors serving as soakaways. Mr. F. C. Spurrell, in Vols. 38 and 39 of the *Archæological Journal*, writes: "The "whole cave is embayed, yet rounded and symmetrical, all combining to give the idea of an excavation seeking space, rather than the material excavated." Mr. Spurrell was apparently correct in his idea that space was the objective of the old chalk hewers—space, that water alone could be expected to occupy. It is sufficiently indicative of anxiety, that when making an addition to a group of drainage pits, care was taken to avoid breaking into an adjoining pit that might have been constructed at some previous time and contained water. Near to the denehole explored by Mr. Jesse Youens was a deposit of very stony clay that for some time had been used for brick making. This clay rested on a level bed of Thanet sand, and was worked out completely in the year 1899. On the surface of the Thanet sand, but so sunk into it that only a portion of each was visible, I had found five Chellian flint implements, and, encouraged by these signs, I moved and examined during many late summer evenings a large quantity of flints that the clay diggers had thrown out of the clay in order, if possible, to find more implements. Not one rewarded my efforts. I must have walked over nearly every part of the pit; children used to play there; the clay-diggers must have trodden heavily on every square foot of the Thanet sand surface. One evening, on visiting this pit, I saw that a circular patch of its surface was missing, and in its place appeared a neatly-made well with smooth sides, sunk through the Thanet sand. Mr. Ernest C. Youens, photographer to our Kent Archæological Society, and his brother Ralph, had the curiosity to descend the shaft of the well and found at the bottom a similar construction to that discovered by Mr. Jesse Youens in 1873. The following is taken from the neatly-kept diary of Mr. Ralph Youens: "Monday, "March 12th, 1900; temperature 48°. On the floor of level bottom of Mr. Kidd's "brick earth pit, south side of Shepherds Lane, a denehole was discovered about "a month ago, the sloppy weather that prevailed in the middle of February having "caused a falling in of the earth and thereby revealing its existence. It was some "little distance south of the other and well-known denehole close to the road, "lately filled up, and which is included in the same quarry. This morning, Ernest "went with his camera to take photos of the hole, and I accompanied him. The "engineman, Mr. Fulker, with whom Ernest is acquainted, brought a ladder and

" a lamp, and with him we both went down into the hole. It seemed to be, at a rough guess, about 20 feet deep to the original bottom of the cave, but to this must be added 18 feet, the depth of the quarry. The shaft is about 3 feet in diameter, round and smooth, no sign of footholes, and at the bottom there was the usual heap of fallen-in earth. At the bottom are three short caves in the chalk, each about 12 feet in length, and 6 or 7 feet high, one running south, the other two east and west, respectively. The sides of these caves are rough,

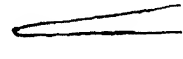
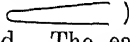
towards the ceiling. This latter is flat and smooth, and is <sup>only</sup> about one foot below the surface of the chalk stratum. The rough sides of the caves are covered all over with the marks of a pointed implement like a pick (of this shape: —  ), all as fresh and perfect as if the caves had only just been worked.

FIG. 2.—FACSIMILE OF ENTRY IN DIARY OF MR. RALPH YOUNG, SHOWING COPY OF PICKMARK.

" and they curve Gothic-archwise towards the ceiling. This latter is flat and smooth, and is only about one foot below the surface of the chalk stratum. The rough sides of the caves are covered all over with the marks of a pointed implement like a pick (of this shape  ), all as fresh and perfect as if the caves had only just been worked. The earth that had fallen down the shaft made an inclined plane reaching to the further end of each cave. While going down the ladder, Ernest picked out a broken flint implement from the side of the shaft. This was our first descent into a denehole, and this specimen, being a very perfect and interesting one, we were delighted with our morning adventure." This denehole was soon afterwards filled up. I must here remark that the diarist thought that a shaft had been made through the clay, but it may be conceded that such a shaft would not have terminated abruptly at the level of the Thanet sand. Again, pick-marks on the hewn chalk looked so "fresh and perfect" that it is difficult to imagine any connection with a shaft sunk in clay through which the rain would have carried particles of the brown clay and stained permanently all the chalk with which it came into contact.

Referring to the triple apses in the deneholes, and especially to the remark of Mr. Ralph Young that one ran "south and the other two east and west respectively," it is worthy of note that a similar arrangement of apses is mentioned in that excellent report of the Grime's Graves excavations, 1914. Page 16: "One of these galleries was 20·8 feet in length and terminated in a *cul-de-sac* in three apses in which were two deer-horn picks for right and left hands." Page 83: "Two and a half feet below this a thin layer of pipeclay can be discerned, and the wall stone layer, some 11 feet below the chalk, is marked by three little recesses or coves."

The appreciation of Mr. Youens of the excellent condition of the pick marks has a corresponding note in the Report, page 17: "The marks in the chalk of the galleries were as fresh as if they had just been made." At page 62 of the Report we read: "Galleries 10 and 16 were worked from our system. The latter had a smooth broad roof which had not fallen, and from its breadth seemed to lead into an extensive area. It terminated, however, in a broad apse." The smaller chamber of the Gravesend Twin Chamber had a slightly domed and smooth roof, and the "ceiling" of the triple apse of the Dartford denehole is described by Mr. Ralph Youens as being flat and smooth. The smoothness in each case suggests attrition by sand-charged water. May I suggest, in passing, that the little holes mentioned at page 61 of the Grime's Graves Report, 1914, as being always on a level with the floor, were more likely intended to let out water than to let in air?

In his 1897 edition of "Flint Implements," page 79, Sir John Evans states that "galleries were found at the base of one of the pits at Cissbury of precisely the same character as those at Grime's Graves and at Spiennes," thus suggesting a contemporary similarity in the methods of work. I believe it is correct to say that in none of the many deneholes explored have any deerhorn picks been found, nor any vestiges of the tools or tackle employed in excavating the pits: everything used has apparently been cleared out with meticulous care. There are no accumulations of potsherds, and working in the chalk must have been thirsty occupation.

During a time of heavy rainfall it may be suggested that owing to its absorbent nature the chalk was saturated to a degree that permitted excavation without making dust. Were any of the picks that were used to fashion our deneholes taken to Grime's Graves where deerhorn picks were found in hundreds, or to Cissbury?

I am reminded of an interesting fact published by Mr. Spurrell in the *Arch. Jour.*, Vol. 38, page 401: "In a denehole at Crayford was found two broad slabs of chalk which were deeply stained with yellow oxide of iron, having white scorings upon them in a kind of rough circular and crossed pattern like those at Cissbury mentioned by General Pitt Rivers and Mr. Park Harrison." Crayford is a village adjacent to both Bexley and Dartford.

The flint implements that I found embedded on the surface of the Thanet sand floor of the brick earth pit were of very ancient types, and could I have known that the brick earth lay over a denehole I would not have taken the trouble to find more implements, as stated.

A bed of brick earth in such a position would represent the youngest member of our geological strata; human skeletons excavated from such clay have proved to differ in no way from those of present-day humanity, although in one case Sir Arthur Keith recognised the tibia as being unusually large.

There is in my collection a six-inch polished stone celt that came, I was assured by the finder, from a good depth in Rutter's brick earth deposit at Erith. Upon repeating the man's statement to Mr. Reginald A. Smith, that gentleman said: "If what you say is true, our prehistory will have to be re-written." The celt is brown stained. Remains of Mammoth were found in the brick earth.

The recent discoveries at Grime's Graves seem to prove that the palæolithic period overlapped the Neolithic in the same way as the Neolithic overlapped the Bronze and the Bronze merged into the Iron Age.

Whatever vicissitudes the flint mines at Grime's Graves may have experienced, their final effacement was probably accomplished by the same watery conditions that caused the last movements of our gravels and brick-earths, thus closing the Pleistocene Period in the Neolithic times somewhere, perhaps, during the latter half of the sixth millennium B.C. It may be that it was during this period that the flint mines at Grime's Graves were worked and towards the close of this period that the real old deneholes were constructed.

W. M. NEWTON.



America, Central: Archæology.

Long.

**The Date of the Maya Ruins at Tzibanche.** By Richard C. E. Long, B.A.

57

In MAN, 1928, 5, Dr. T. Gann figures a wooden lintel discovered by him at Tzibanche, and suggests that the date on it should be read as 9 Ahau 3 Yax, assumed to be the period ending 9-15-2-0-0 9 Ahau 3 Yax. At the same time, Dr. Gann, while agreeing with this reading of Dr. S. G. Morley's, does not accept it as the contemporaneous date, seeing that the stela discovered by him at Chetumal Bay in the same region has the date of 9-8-0-0-0.

I think, however, that the reading proposed is not correct. The day-number in glyph 3 is 9 and the month-day in glyph 4 is 3, as in each case there is just room for an extra dot, which would give these readings. The day-sign in glyph 3 is probably Ahau, though it is not very distinct. But it is very doubtful if glyph 5 is a period-ending. There is no numeral and no distinct tun or other period, and without these the sign resembling an ending sign has no meaning. In any case the use of the ending sign with tuns seems confined in the inscriptions to 13-tun endings. No other tun-endings were usual except the hotun and lahuntun endings, which have special signs. The month-sign in glyph 4 is quite clearly either Uo or Zip, as it shows the two crossed bands found in the signs for these months only, while the superfix resembles Zip more than Uo, though these two month-signs are always hard to distinguish. We have then either 9 Ahau 3 Zip or 9 Ahau 3 Uo, most probably the former. These being calendar round dates, there is no difficulty in reconciling them with the Chetumal Bay date. Their possible occurrences in the first half of Cycle 9 are:—

9-1-1-14-0	-	-	-	9 Ahau 3 Zip	9-2-2-0-0	-	-	-	9 Ahau 3 Uo
9-3-14-9-0	-	-	-	„ „	9-4-14-13-0	-	-	-	„ „
9-4-6-7-4-0	-	-	-	„ „	9-7-7-8-0	-	-	-	„ „
9-8-19-17-0	-	-	-	„ „					

The most probable contemporaneous dates in view of recent discoveries are either 9-6-7-4-0, 9-8-19-17-0, or 9-7-7-8-0. Closer than this it is not safe to go in the absence of dates at other sites in the neighbourhood which might give a clue.

The distance number in glyph 1 is, no doubt, 4-18, and the sign in glyph 2 may be 2 tuns, though this is doubtful. But neither 4-18 nor 2-4-18, if read either backward or forward from 9 Ahau 3 Zip or from 9 Ahau 3 Uo, give a date of any significance.

In conclusion, it may be noted that glyph 3 (under half) is probably the secondary series terminal glyph (S. G. Morley, "The Inscription at Copan," p. 257).

RICHARD C. E. LONG.

Archæology.

Moir.

**The Neanderthal Phase of Man.** By J. Reid Moir.

58

In the recently delivered Huxley Memorial Lecture for 1927,\* Dr. Aleš Hrdlička makes certain statements upon which I desire to comment.

Page 253. It is suggested that the mammoth first appears in the Acheulean culture stage. Typical teeth of this creature are, however, found in the Cromer Forest Bed, which is of Chellean age, and separated from the Acheulean and the Mousterian epochs by the immense glaciation represented by the Tills and Contorted Drift of the north-east coast of Norfolk.

Page 253. It is held that "the cultural remains of the Mousterian in the open stations . . . lie mostly in old gravels." All such remains occurring in gravel

\* Journ. Roy. Anthr. Inst., Vol. LVII, 1927, July to December.

are of necessity derivatives, and the place where they are thus found cannot be regarded as a "station," but merely the accidental site of their deposition by water-action.\*

Page 253. "There is no evidence of any critical geological manifestations "either about the beginning or about the end of the Mousterian period." Towards the close of this epoch, the Third Glacial Period of East Anglia took place, resulting in the deposition of thousands of square miles of Upper Chalky Boulder Clay, and among other geological changes the erosion of the Fenland, 1,306 square miles in extent.†

Pages 253-6. It is stated that two or three cultural stages, or horizons, of the Mousterian culture were found at Ipswich, indicating a repeated occupation of the same site after shorter or longer intervals. I know of no such site within the boundaries of the town of Ipswich.

Page 258. "The (Mousterian) technique is partly different from, but is in "general not inferior to either the late Acheulean, or the earlier Aurignacian." The true Mousterian technique is fundamentally different from that of either the late Acheulean or the earlier Aurignacian, owing to the fact that Mousterian man generally delivered his flake-removing blows at an angle differing from that adopted by the other prehistoric peoples mentioned. The result of this difference is plainly observable in the type of flake-scar produced, and can be demonstrated experimentally. In the earlier portion of the Mousterian epoch, the hand-axe is still present, though, even so, exhibiting clear evidence of Neanderthal man's handiwork. But this well-known type of implement begins from then to get ever less in number, and is replaced by the Mousterian *racloir* and *pointe*, which are definitely primitive in their conception,‡ and hark back to the pre-Chellean, and even to the Eolithic culture stages. There is no real basis of comparison between the magnificent hand-axes of the Late Acheulean and the workmanship of Mousterian times. Nor is it possible to conceive how the specialised technique of this epoch developed into the "channelled" flaking of the Aurignacian. The presence of hand-axes in the Early Mousterian deposits, and of Aurignacian-like forms of implements in the later beds of the Mousterian culture-stage, can be explained on the supposition that the Neanderthal people copied the dominant type of implement in use among the Acheuleans, and were influenced in a like manner towards the close of their career by the Aurignacians. But the Mousterian technique is one of the most clearly-defined of all methods of flaking in vogue in prehistoric times. It is also primitive in its conception, and definitely below the standard of that of both Acheulean and Aurignacian times. It is thus not possible to accept Osborn's statement (quoted on p. 258) that the Mousterian "constitutes "a further evolution of the two earlier cultures" (the Chellean and the Acheulean), if by "evolution" improvement is indicated.

Page 258. "The use of bone begins in the Mousterian." Very definite bone implements have, however, been found in (a) the Piltdown Gravel,§ and (b) the Suffolk Bone Bed beneath the Red Crag.\*\* Each of these deposits pre-dates those of the Mousterian by an immense period of time.

Page 271. "The Penck-Brückner conception of the Ice Age as composed of "four distinct periods of glaciation with three well-marked inter-glacial periods, "does not harmonise with either the palæontological or the human evidence."

\* Moir, J. Reid. "The Antiquity of Man in East Anglia." 1927, p. 40.

† Moir, J. Reid. *Journ. Roy. Anthr. Inst.*, Vol. IV, 1920, January to June, pp. 135-152.  
Moir, J. Reid. "The Antiquity of Man in East Anglia," 1927, p. 63.

‡ Moir, J. Reid. "Pre-Palæolithic Man." W. E. Harrison, Ipswich.

§ Dawson, Charles, and Smith, Woodward A. *Q.J.G.S.*, Vol. LXXI, Part I, 1915.

\*\* Moir, J. Reid. *Proc. P.S.E.A.*, Vol. II, Part I, pp. 116-131.

While this may appear to be the case in non-glaciated countries, it is otherwise in East Anglia, where four glacial epochs, with three well-defined inter-glacial periods, together with their associated flint implements, seem clearly defined. Incidentally, it may be stated that the Early Mousterian implements of this area are found in beds laid down during the later part of the Second Inter-glacial episode.

Page 273. "There is no evidence of the pre-Aurignacian whereabouts and "the doings of *H. sapiens*." There is, however, some considerable evidence in existence of the probable whereabouts of *H. sapiens* in pre-Aurignacian times,\* but it is not accepted by Dr. Hrdlička and others, presumably because, when human remains showing "modern" characteristics have been found in pre-Aurignacian deposits, they are rejected as of unproved antiquity. But, as no less than four examples of Neanderthal man, found under non-scientific circumstances by workmen, are included in Dr. Hrdlička's list of genuinely ancient human remains, it cannot be that all the examples of *H. sapiens* discovered in pre-Aurignacian deposits are disbelieved in solely because of the alleged uncertainty of their provenance.

I share with Dr. Hrdlička a feeling of difficulty in understanding exactly how or why Neanderthal man became extinct, but I find it still more difficult to believe that the typical Neanderthal man of Upper Mousterian times of La Chapelle† was evolved into *H. sapiens* in the time available between this latter cultural stage and that of the Early Aurignacian.

J. REID MOIR.

## REVIEWS.

Archæology: Metals. Friend: Garland: Bannister.

*Iron in Antiquity.* By J. Newton. 59  
Friend. Crown 8vo. Pp. 221.  
Illustrated. Griffin & Co. 1926. Price  
10s. 6d.

*Ancient Egyptian Metallurgy.* By H.  
Garland and C. O. Bannister. Crown 8vo.  
Pp. 214. Illustrated. Griffin & Co. 1927.  
Price 12s. 6d.

The importance for the history of civilisation of a study of ancient metallurgical processes is now well recognised, and it is an encouraging fact that metallurgists are showing an increasing interest in the historical aspects of their art, of which the two books before us are an illustration. Dr. Friend's volume is an expansion of his Carnegie Scholarship Memoir, submitted to the Iron and Steel Institute, and gives in broad outline a survey of the history of iron and steel in ancient times. Whilst archaeologists have adopted the view that iron was later than the Bronze Age, many metallurgists have maintained that the knowledge of iron-smelting existed at a much earlier period than this view would admit, basing their claim on the fact that the reduction of iron from its pure ores takes place at a comparatively low temperature, and that such ores are abundant and easily recognised. The absence of iron objects from early deposits they explain by the rusting

of the metal in the earth. On the other hand, iron objects of great age have been found which are sufficiently well preserved for examination, or have been converted into oxide of so coherent a form that the shape of the original object can be recognised. Although iron ores are easily reduced in a furnace of the prehistoric type, the metal is not obtained, as are copper and bronze, in a molten condition, but in a spongy mass which has to be heated, exposed to air, and repeatedly hammered, before it becomes useful metal, and it may well be that this art was only acquired late. The few objects of iron of undoubtedly early date which appear to contradict this view of its late origin may have been forged from meteorites, and the evidence for a widespread use of this material was presented by Zimmer in 1916. Dr. Friend accepts this evidence, and does not claim an exaggerated antiquity for the smelting of iron. The place of iron in legend and tradition is discussed, the references in Homer and in Herodotus being reviewed in detail. For the Roman period there is an abundance of evidence, and the author devotes special attention to Roman Britain, giving many analyses. It is much to be desired that careful analyses of iron objects of earlier date should be made, especially as regards the presence of nickel, which in quantity indicates a meteoritic origin. Dr. Friend

\* Keith, Sir A. "The Antiquity of Man." 1st edition, 1915.

† Bouyssonie et Bardon. *L'Anthropologie*, T. XXIV. 1913.

takes particular interest in the Scandinavian records, and gives numerous quotations from the sagas. He is, however, in error in supposing that swords were ever enclosed in a hard layer of oxide or mill scale, which protected them from rusting. Such layers, when found on ancient iron objects, have been produced by subsequent oxidation, and were not present originally. The author has himself examined British iron masses, and records his observations. India furnishes interesting examples, and it is still a mystery how such relatively large columns as those of Delhi and Dhar were built up by the welding of such lumps as could be made in the primitive smelting furnaces. Egypt and Mesopotamia are considered in turn, and the absence of iron from the remains of ancient Mexico and Peru is duly noted, the existence of carvings in hard stone in America being used as evidence for the opinion, often disputed, that the Egyptians did not need iron tools for their carvings. Dr. Friend gives many references to papers, so that his book, though elementary, will be found useful by the student. It is commendably free from rash speculations, such as have marred some essays on the early metallurgy of iron.

Professor Bannister has edited the notes left by the late Major Garland, accumulated in the course of his chemical and metallurgical studies in Cairo. It is unfortunate that so few analyses are given, as a full knowledge of the composition of the objects is of the highest importance in deciding as to their origin. Photo-micrographs are also included, but some of these are disappointing, which is perhaps to be attributed to the decayed state of the fragments examined. Where sound metal is available, it is possible to draw very definite conclusions as to the method of manufacture and of subsequent treatment. The author's conclusions must be accepted with caution, as many problems in Egyptian metallurgy remain obscure. On the vexed question as to whether iron tools were used in Egypt (a few early sporadic examples, probably of meteoritic origin, excepted), he takes a view which is the opposite of that accepted as probable by Dr. Friend, holding that only with hardened steel tools could the sharply-incised figures and hieroglyphs have been cut in hard diorite and similar stones. Whilst the evidence adduced is by no means convincing, it must be admitted that the author has given good reasons for supposing that the methods hitherto suggested, such as sawing and drilling with copper tools fed with emery, would not suffice for the production of perfectly sharp incised figures, meeting the base at a right angle. It

would seem that experiments are necessary to supplement the archaeological evidence. In this field, the co-operation of archaeologists and metallurgists is essential if any definite conclusions are to be reached.

C. H. DESCH.

Sociology. Crawley; Besterman.

*The Mystic Rose, a Study of Primitive Marriage and of Primitive Thought in its bearing on Marriage.* By Ernest Crawley. A new edition, revised and greatly enlarged by Theodore Besterman. In 2 volumes. London: Methuen & Co. 1927. Pp. xx + 375; vii + 340.

"The ultimate appeal in these questions [matriarchy, marriage by capture, primitive promiscuity, and communal marriage] is to universal facts of human physiology and psychology." "Survivals are not fossil forms in living culture, but represent ideas which spring eternally from permanent functional causes." "Human nature remains fundamentally primitive." "Primitive thinking does not distinguish between the natural and the supernatural." "The importance of subconscious thought can hardly be estimated."

It may seem strange that such sound principles should have been expounded a quarter of a century ago, and yet have had so little influence on anthropology that the field workers of to-day feel it necessary to insist that before explanatory theories of human customs be offered, such customs must be examined in relation to their functional value, both conscious and unconscious, in any given society. However, a careful perusal of "The Mystic Rose" will convince the student that the apparent neglect of such statements is due to the nature of the work itself. Mr. Crawley conceived a brilliant idea concerning the functional value of marriage ceremonies—that they are performed in order to counteract the danger due to the contact of the sexes. According to Mr. Crawley, all human contacts are fraught with danger: this "egotistic sensibility" is intensified by strangeness, therefore the first time of performing any act is dangerous, but the difference of sex is in itself the most intense form of strangeness and therefore heightens the danger of the first union of the sexes, and thus gives rise to the need for further caution. Hence the sexual taboo arises, with its accompanying ceremonies. But this is not all; the idea of contagion, which Mr. Crawley believed to be inherent in the earliest human thought concerning sex, is the basis of both the moral and social idea, the foundation of human society.

Before examining this far-reaching theory we must consider his ideas concerning human contacts. "The individual *qua* "individual is potentially in danger from "other individuals and dangerous to "them." Yet the individual does not appear, like Minerva, fully grown; his first human contacts are those with the mother, insuring warmth, food, and protection. These pleasurable contacts must remain in the memory subconsciously, even if they should be obliterated later in life. The contacts of early babyhood, equally with those of infancy, bring about the protection of the individual by other individuals from the hardships of the world; and this is so even if we consider a hypothetical group so primitive that, in common with other mammals, it disrupts at the maturity of the young. Hence, though the adolescent or adult individual may form contacts with strangers necessarily fraught with danger, he has already had experience of human contacts with both sexes which were not primarily dangerous.

Mr. Crawley's theory of civilisation (for it is in reality no less) starts from the hypothetical idea of a fully-grown adult male (note the sex, for the danger of contagion with the opposite sex is not equal in both sexes—a man has something to lose from the contact; the woman has something—masculinity—to gain) and this male presumably is unpractised in such immature experimentation in sex as is common both to animal and human nature. This fact alone vitiates Mr. Crawley's theory. But it is harder to understand how Mr. Crawley came to the conclusion that the sexual taboo is upon *all* intercourse primarily, and that it came only secondarily to create the barriers of incest and exogamy, which he considers useful to mankind in assisting "Nature's institution of the family," and in "producing the marriage system, by preventing licence both within and without the family limits, keeping men from promiscuity and incest, degradations which were never primitive . . . but also of emphasising the characteristic qualities of each sex" (Vol. I, p. 260.) It is impossible here to follow all Mr. Crawley's arguments. He gives no evidence for suggesting that unchastity itself was a sin before incest was considered in this light (except the derivation of the English word from the Latin); and it seems strange that the very act which is common to the animal and vegetable kingdom alike, and is as necessary to life as every other natural function, should have been considered sinful, except in an advanced society where the family was already a stable social unit. But according to this

theory, it was in the most primitive family that sexual taboos arose and ceremony (or the beginning of religion) was required in order to permit the *natural* mating of a pair in the *natural* family, for Mr. Crawley says: "there was, of course, a time when "there was no marriage service, *but the "ideas of such were latent in the actual "union of man and woman"*\* (Vol. II, p. 259). Mr. Crawley has not attempted to show how only one adult pair in the family discovered the magical means of breaking the sexual taboo and so instituted marriage, or why the other members submitted to sex taboo and so became protected by it from the temptations of incest, promiscuity, or licence. Nor is it clear why, if religious content is present in latent form in the union of man and woman, it should be denied to the rest of the animal kingdom. In truth, far too little attention is paid to the fact that immature and prenuptial sexual freedom is common in savage society and does not appear to be dangerous. Emphasis is rightly laid on the first nuptial intercourse, which does need to be safeguarded, but it should be recognised that this is a social act which requires social (and therefore religious) sanction, and is not an instinctive mating.

The work is marred by numerous dogmatic and arbitrary statements, either entirely unsupported or inadequately supported by evidence, such as "Incest in "primitive thought was not differentiated "from any sexual connection"; "Obscenity is either unknown among savages "or regarded as a heinous sin." Whereas the truth is that each society (and even social grades within a given social system) has its own standards of decency. Again: "A gift . . . to primitive man . . . "is part of himself"; "A wife in early "thought is part of the man," etc. This last statement, apart from the lack of corroboration, is entirely at variance with Mr. Crawley's main theory, that of contagion and sex antagonism—a theory which, had it not been pressed too far, would have been fruitful and suggestive; for the mutually ambivalent attitude of the sexes has created a problem which neither magic nor religion has ever entirely solved, a problem continually shifting with social and economic conditions.

Mr. Besterman's editing is most painstaking, but it may be doubted whether there has been substantial gain by the enlargement of the work to two volumes.

BRENDA Z. SELIGMAN.

\* The italics are mine.

Malay Archipelago :  
Ethnography.

Krämer.

*West-Indonesien : Sumatra, Java, Borneo.* Von Professor Dr. Augustin Krämer. [Vol. III of the "Atlas der Völkerkunde."] 12 x 9. Pp. 104 (including 45 illustration plates and 2 maps). Stuttgart : Franckh'sche Verlagshandlung [1927]. Price 16.50 marks.

61

The introduction (pp. 7-11) of this work contains theories as to the settlement of the Indonesian islands (and the remoter parts of Oceania) and the origin of the various types of culture to be found there, for the discussion of which there is no space here. The suggested identification of the dark races of Melanesia, New Guinea, and even Australia, with those of Africa and also the Negritos of the Andamans, Philippines and Malay Peninsula, seems at least premature. The term "alt-malaiisch" for the earliest of the immigrant Asiatic races and cultures is unfortunate, for there is no reason to suppose that the Malays, properly so called, were the first arrivals from the mainland. Nor is it by any means certain that they were patriarchal. Their near relatives, the Minangkabau people, are to this day most typically matrilineal; and there are traces of mother kin in other parts of the region. The distinction made between an Indonesian and an Indian culture (both being supposed to have come from India, but the former chiefly from Further India) is not very clear. There is much else, especially in relation to boat-building and architecture, that is suggestive and deserves careful consideration.

In the body of the book the odd pages 13-101 are illustration plates of line drawings, facing the explanatory text. Besides the islands mentioned in the title, the work includes the smaller ones near Sumatra and Java, the Hova part of Madagascar, and the Andamans and Nicobars, but not the Malay Peninsula. The omission from the Sumatran part of a section dealing specifically with the Malays, properly so called, is remarkable, even though they are "stark vermischt" (p. 50). Otherwise, there is much valuable ethnographical information in the text; and special mention must be made of the bibliographical references, which add greatly to the value of the book.

C. O. BLAGDEN.

India : Folklore, Bodding.

*Santal Folk Tales.* By P. O. Bodding. Oslo : Instituttet for Sammenlignende Kulturforskning, Vol. I. 1925. Pp. xvi + 369. Vol. II, 1927. Pp. viii + 403.

62

The Scandinavian Lutheran Mission, which began work in the Santal Parganas

in 1867, is interested in the culture of the folk among whom it works, and Mr. Bodding's writings in the Memoirs and Proceedings of the Asiatic Society of Bengal and elsewhere are well known. It was his collection of nearly two hundred folk tales which C. H. Bompas translated, in abridged form, in his "Folklore of the Santal Parganas" (1909). A number of those tales reappear *in extenso* in the volumes under review, with the vernacular (in roman) on the left, an English translation on the right, and notes below—an ideal arrangement which might well be adopted in epigraphic and other work.

Of the 24 tales recorded, 15 are about jackals, and 9 about women; 37 are classed as "humorous tales," and 6 are about "ogres." The notes, which are concise but sometimes redundant, are a mine of valuable information; some of them may seem obvious to those who are familiar with Indian life, but they are, no doubt, a necessity to those who know India only in print. Verbal descriptions of domestic utensils are always difficult to follow, and a plate or two to illustrate the main types in use would be helpful. The translation, though not always quite idiomatic, is clear and conveys a lively impression of Santal mentality.

Professor Sten Konow, in an all-too-brief foreword, draws attention to the dual rôle, due perhaps to different cultural ingredients, of the jackal, who in some tales is an effective helper of those in trouble, in others, like the devil of European folklore, a futile mischief-maker. Dr Sten Konow discusses the "aryanization" of Santal culture as revealed in the tales. His description of the Santals as "a primitive Kolarian tribe" is not happy; "Kolarian" was long ago discredited as a term for the Munda branch of Austrie languages to which Santali belongs, and a people of over 2½ million souls is too big to be called a "tribe." Nor are Santals "primitive," for they first appear in the Santal Parganas towards the end of the eighteenth century, since when they have been in close touch with Hinduism and the British Raj; they have spread to the Bengal Plain and the tea gardens of Assam; a third of their number were returned as "Hindu" in the Census of 1921; of Mr. Bodding's informants, one was 30 years in his service, the other was a schoolmaster who died in Mesopotamia during the war; and the tales themselves are saturated with Hindu influence. Against Christian influence reaction began as far back as 1871 in the "Kharwar movement," which is still alive. Folk tales, after all, are illustrations rather than the text of cultural history, and a thorough all-round study of the Santals,

like those of S. C. Roy on their neighbours, is needed before their tales can be properly appraised.

F. J. R.

Tibet: Ethnography.

Francke.

*Antiquities of Indian Tibet.* Part II: *The Chronicles of Ladakh and Minor Chronicles.* By A. H. Francke, Ph.D. Edited, with foreword, by F. W. Thomas, M.A., Ph.D., Archaeological Survey of India, New Imperial Series. Vol. L. Price 45s.

63

The chronicles of Ladakh, the text, translation and annotation of which form the bulk of this volume, are divided into ten parts, the last two of which deal with the period of the Dogra Wars up to the year 1886 A.D., and are the work of a modern historian, Murohi Dpal-rgyar of Leh. The rest of the chronicle is edited from five manuscripts—Schlagintweit's MS., Marx's three MSS. and a MS. in the British Museum. Introductory verses in which the pre-Buddhist Gesar is hymned together with Mañjuśrī, are followed by a cosmology and cosmogony. On the whole, these are Indian in derivation and end with a confused account of the seven Buddhas. However, here as elsewhere throughout the earlier chronicles, the influence of pre-Buddhist, Bon-po folklore is evident. The third book deals with the genealogy of the Śākya, from whom the kings of Ladakh claimed descent; this portion seems to be directly derived from the Tibetan Lalitavistara. This legendary preface over, the dynasties of Great Tibet, Western Tibet and of independent Ladakh are treated in order. It seems that the first historical king mentioned is Sron-Btsan-Sgam-Po [600-50 A.D.]

After 1400 the chronicles expand and epigraphical confirmation is not wanting.

However, their nature is always monastic and the range of interest limited. The later parts are valuable when read with the accounts of Moorcroft, Henderson and Vigne, the frequent topographical notes being excellent. As a whole, the book is a useful addition to Cunningham's great work, but one rather wonders if annotated dynastic lists would not have been equally useful. However, the book contains plenty of material for future amplification, which one hopes Dr. Francke intends. One wonders a little why such a work should be published by the Archaeological Survey of India.

K. DE B. C.

Britain: Archaeology.

Cunnington.

*The Pottery from the Long Barrow at West Kennett, Wilts.* Compiled by M. E. Cunningham. Small 4to. Pp. 19. 13 plates. Devices: Printed, for private circulation, by George Simpson & Co. 1927.

64

The pottery of the Neolithic Age from sites in England and France is at last beginning to receive the attention it deserves. Both in this country and in the north-east of France the remains are scanty and fragmentary, and before a careful comparative study can be made it is necessary that full illustrations should be available for students. Among those found in this country there are few of so great importance as the potsherds found in 1859 by Thurnam in the long barrow at West Kennett, and Mrs. Cunningham has laid archaeologists under a great debt of gratitude for having produced this little volume, in which are portrayed all the fragments found in that barrow, except the six pieces originally illustrated by Thurnam. The photographs and the explanatory text leave nothing to be desired.

H. J. E. P.

## CORRESPONDENCE.

Ethnology.

Hutton.

"The Children of the Sun."

To the Editor of MAN.

65

SIR,—In the November number of MAN Mr. Perry challenges me to reply to certain points in which he discusses some reflections of mine on his "Children of the Sun."

He has asked for an example of identical results from different origins. I give the first four that occur to me. The fire-piston was invented in France about 1800 as the result of laboratory experiments with compressed air. It is also found over a wide area in eastern Asia from Yunnan to Indonesia among people ignorant of the properties of condensed air who cannot,

without evidence, be supposed to have obtained it as the result of a similar process of experiment. Mr. Thomas, of the Oxford University Museum, reported in a recent number of MAN his personal discovery, as a boy, of the bull-roarer in the form of a paper-knife tied to a string; this in itself shows that we cannot safely assume that the present distribution of the bull-roarer is the result of diffusion from a single source. The poisonous property of adrenalin was discovered by physiologists investigating the endocrine secretions of the human body, and then artificially produced by chemists. It was independently discovered by Indians of the Amazon, who use the adrenalin secretion of the glands of a certain toad



as an arrow poison. The Naga medicine-man believes in sympathetic magic; like causes like; to cause rain he pretends that rain is falling. On this principle he pretends to extract foreign matter from his patient's body in order to effect the removal of the disease. The patient, believing that the very cause of the disease has actually been extracted, is cured by suggestion. Even so in some forms of modern therapeutics suggestion is deliberately used to cure disease, but the observation, deductions and theory leading to its use are entirely other than those of the medicine-man.

Mr. Perry says that "warfare is an organised form of behaviour, and is not 'natural' to mankind." Obviously warfare is a form of organised behaviour. What he has to demonstrate is that such forms of behaviour cannot become organised in different regions independently, and are not merely responses to conditions of environment which may recur at various times and places.

As regards the dual organisation I am accused of having tangled up various theories. Certainly I have put forward more than one view of what I regard as different phenomena but which Mr. Perry does not. It is he who has done the tangling. He now states categorically that "the fact remains . . . that the 'duality of territory, of villages and of 'offices all belong to the same system, 'and have been propagated together.'" The Sangtam tribe is located in two groups about two river valleys divided by a high watershed; this geographical division is therefore symptomatic of the Dual Organisation, in spite of the fact that the Sangtam villages on the watershed making the tribal area a single continuous unit were driven out by Semas and others within living experience. So, too, the disposition of the Rengmas in the Naga Hills and across the Dhansiri River in the Mikir Hills is a case of this Duality again; but we know that the latter group was only detached from the original stock and pushed out by other tribes about a hundred years ago. My quarrel with "The Children of the Sun" was the (as it appears to me) arbitrary interpretation of irrelevant data to make them fit the rather Procrustean bed of Mr. Perry's thesis. Some (not all) of the cases cited by him as evidence of dual organisation are, I still maintain, merely geographical and of no relevant significance. An apposite instance of the uncritical use of data is to be found in para. 4 of his letter. A story exists among some Kukis and some Nagas ascribing a common origin to Kukis, Nagas and Manipuris. The royal house of Manipur

claims descent from the snake Pakhangba. Therefore Kukis and Nagas in general claim descent from a snake. This, Mr. Perry says, is how he reads the evidence; but the syllogism is bad in logic and the conclusion happens to be contrary to the facts. It is true that he has the case of the Anals in his favour, an Old Kuki tribe living alongside or among the Manipuris and under their domination. He claims also the Thado, but if he will refer again to the authority quoted by him he will see that Col. Shakespear only says that the Anals state that they and the Thado share the Manipuri connection with Pakhangba. The Thado do not do so, and the Nagas seem quite out of it anyhow. Another instance of method: Mr. Perry splits up the fourth paragraph of my former note, a paragraph obviously intended to be read as a whole, criticises the first part of it as "begging the question" to make a forensic point, and speaks of the latter part as "a subsequent remark." As for "being aware of the possibility of 'diffusion,' I never heard . . . of a biologist who was not. Everyone knows that diffusion of culture is an universal phenomena, but the mere fact that it is so postulates the existence of some such common basis of human behaviour as "evolutionary" theories depend on. Otherwise why should the anthropoid apes be out of it? Similar stimuli may and sometimes, at any rate, do produce similar reactions in the human being. The laws of real property in feudal Britain, in Ashanti (*vide* Rattray, "Ashanti," p. 223), and, in a lesser degree, perhaps, among the Sema Nagas give an instance of this. Diffusion is precluded, at any rate in the first and third cases, where the developments of the laws referred to from other conditions is a matter of known fact.

The question of whether or not a given item of culture is the result of contact or of independent development, must be decided, if at all, on the evidence available in that particular case. A working hypothesis is a very useful servant, but a dangerous master, and the investigator not only *has* a logical right to consider piecemeal alleged elements of dual organisation, or any other data on which an hypothesis is constructed, but must do so if he is to examine the validity of that hypothesis. Mr. Perry has advanced the ingenious and attractive hypothesis of the Archaic Civilisation, but he must not expect us to accept it as proved without scrutiny of the evidence put forward to prove it, nor, if it be really true, can any scrutiny damage it.

Yours faithfully,

Kohima, Naga Hills. J. H. HUTTON



Sweden: Anthropology, Physical. Morant.  
**The Racial Characters of the  
 Swedish Nation.**

To the Editor of MAN.

66

SIR,—In a letter to MAN (1928, 25) Professor Lundborg has repudiated a suggestion made by me in a review of "The Racial Characters of the Swedish Nation" (1928, 11) to the effect that no adequate definitions are given of the measurements of the living used in that work. He is wrong in supposing that I had overlooked the statement made on p. 9 that "all measurements are taken according to Martin's 'Lehrbuch der Anthropologie,' 1st edition." In referring to Martin's measurements, it is customary—and, indeed, absolutely necessary—to quote either the numbers in his list, or their German names (or, in some cases, the French or English equivalents provided). Neither practice was followed in this case. All we are told of five measurements is the following, and several others are dismissed as summarily:—

"*Head length* offered no great difficulties in measurement. The same is true of "*head breadth, minimum frontal diameter, face breadth, and bigonial diameter.*"

But Martin defines eight head lengths (Nos. 1, 1a, 1b, 1c, 1d, 2, 2a and 2b) and four facial breadths (Nos. 7, 7a, 10 and 10 (1)) other than the bigonial. It is true that it is possible to infer precisely what some of the measurements are from passing remarks made in the descriptive part of the text, or from the list of anthropological terms given on p. xiii, but others are left uncertain. All doubts would have been removed if sufficient references or definitions had been given in the section on the "Collection of the Material." A number of workers who have followed Martin have found it necessary to add notes explaining in detail the ways in which they interpreted the definitions of

some of his measurements. None such are given here; e.g., nothing is said of the degree of pressure used in finding head and body diameters. Under these circumstances, I still feel that it was no exaggeration to say that the authors of "The Racial Characters of the Swedish Nation" provided no adequate definitions of their measurements of the living.

G. M. MORANT.

Africa: Ethnology. Crawford.  
**The Boats on the Victoria Nyanza.**

To the Editor of MAN.

67

SIR,—The argument between Professor Elliot Smith (MAN, 1928, 41) and Mr. James Hornell (*ibid.* 1) should be capable of settlement, since it is about facts which can be demonstrated. Mr. Hornell alleges that Professor Elliot Smith made two alterations in copying Stanley's drawing; and that these alterations "create" a fictitious parallelism between the "African and the Scandinavian type." In other words, Mr. Hornell alleges that the alterations reinforced the argument.

Professor Elliot Smith's remarks in your last number do not correct this impression. They do not explain how "what we now realize to be a decorated "string" was converted into a stout pole. Stanley's drawing looks to me quite plain and obvious; if it were not, Sir Harry Johnston's words at any rate are, and they can only mean one thing ("the top of the "prow . . . is steadied by a stout "rope being carried tightly from the "uppermost point of the prow to the nose "or beak of the canoe.").

The point at issue is—how did so important a modification arise? This is a matter which your readers can decide for themselves if you will, in your next number, reproduce side by side both Stanley's drawing (from the block on p. 59 of your last number) and Professor Elliot Smith's copy of it.

O. G. S. CRAWFORD.

## ANTHROPOLOGICAL NOTES.

*The Huxley Memorial Medal for* 68  
 1929.—The Council of the Royal Anthropological Institute has awarded the Huxley Memorial Medal for 1929 to Baron Erland Nordenskiöld of Göteborg. He has also been invited to deliver the Huxley Memorial Lecture in November of that year. Baron Nordenskiöld's researches in the archaeology and ethnology of South America hold a high place in the estimation of the scientific world, and the award will be welcomed as a merited recognition of many years' valuable work.

*The International Congress of Americanists*, 1928.—The announcement is made that the XXIII International Congress of Americanists will be held in New

York City during the week beginning September 17th next. An Organising Committee has been formed, of which Dr. Franz Boas is Chairman, and Mr. P. E. Goddard of the American Museum of Natural History, is Secretary. The Congress will be divided into 6 sections, dealing with the Ethnology of America, the Archaeology of America, the Origin, Distribution, and Ethnography of the American Indian, Native Languages, the Discovery and Early History of America, and, finally, Geographical and Geological Questions with special reference to human activities. The titles of papers and abstracts for submission to the Congress should be in the hands of the Secretary not later than June 1st.



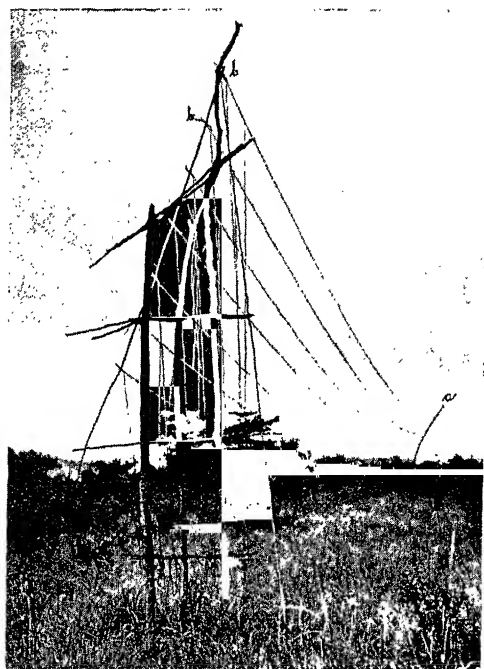


FIG. 1.

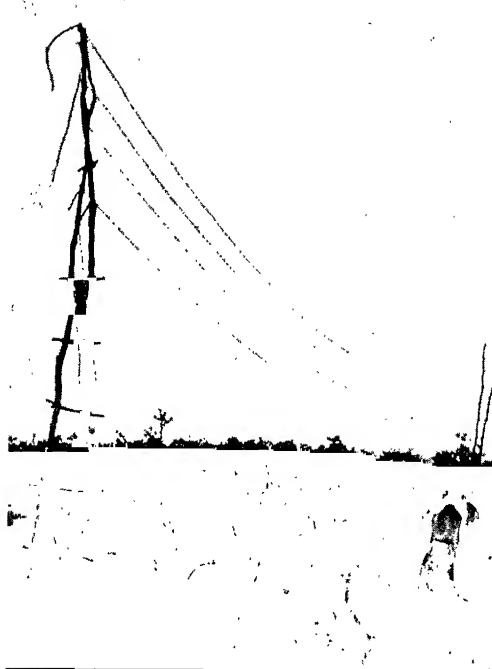


FIG. 2.



FIG. 3.

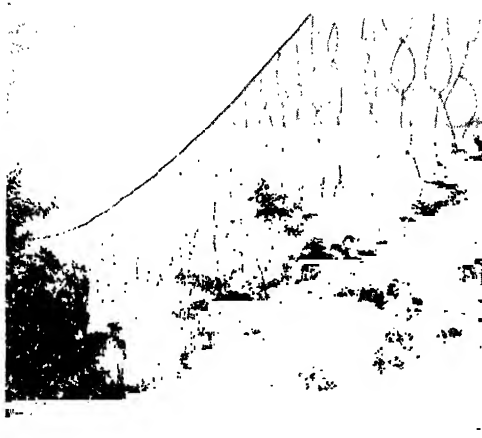


FIG. 4.

A NOOSE-TRAP APPLIANCE FOR THE CAPTURE OF THE LARGE  
FRUIT-EATING BATS OF THE LOWER CONGO REGION.

## ORIGINAL ARTICLES.

With Plate F.

Congo: Technology.

Lindblom.

**A Noose-Trap Appliance for the Capture of the Fruit-eating  
Bats of the Lower Congo Region.** **69**  
*By G. Lindblom. With Plate F.*

Practically nothing has been published, as far as the writer is aware, in the way of descriptions or depictions of a very interesting apparatus used by certain tribes on the Lower Congo river in capturing the large frugivorous bats, *Rousettus aegyptiacus* and *Eidolon helvum* (*Pterocyton helvus*), the toothsome qualities of which are highly esteemed by the natives. Recently, however, I have received from two Swedish missionaries in the Congo—the well-known Congo student, Dr. K. E. Laman, and his colleague, the Rev. J. Hammar—valuable information as to the methods employed in the trapping of those animals, together with some instructive photographs of the snaring apparatus, and am thereby enabled to adduce this slight contribution to our knowledge of native trapping methods on the Lower Congo.\*

My informants have found these traps on the northern bank of the lower course of the Congo, *i.e.*, the district about Brazzaville, Madzia, Musana, Mindouli, etc., in French Congo. The traps appear to be chiefly used by the Bateke, though they are also found among the Bakongo and the Basundi. Quite possibly the last-mentioned tribes have borrowed it from the Bateke, as, according to Dr. Laman, the appellations used by them for the apparatus as a whole, as well as for its various component parts, are words of the Teke language.

The edible bats are called *n'gembo* (pl., *ban'gembo*)—according to Bentley, "a large bat." In the daytime they always rest in dense brambly vegetation, or other dimly-lit places; but after sundown they begin to bestir themselves, and then issue forth in great swarms, making for places where they may find the kind of fruits for which they have especial liking. On one occasion Hammar saw thousands of them coming out of a wooded valley. When they have eaten their fill, they flock back to their hiding places. Then they habitually pass through forest glades and down depressions in mountain ridges between forest-grown valleys, and it is in such places that the trapping contrivances are staged—that is to say, in passes, or openings, between forest areas.

Plate F, Fig. 1, shows a trapping contrivance from the Madzia district. The far end of the rigging consists of a long pole (*a*) guyed down with a rope. The cross-pieces on the near part of the framework (which is also steadied by stays) serve as rungs of a ladder for arranging the snares, or when it is necessary to repair the apparatus. Between these pole-erectations several rows of supple lianas are stretched, with one end directly attached to one end of the frame (here the far one), whilst at the other they are continued by a string which runs through a loop formed by a liana (*b*). These loops, which are attached to a slanting stick, act as pulley-blocks through which the strings are run down to the lowest cross-piece, where they are made fast. By means of these strings the lianas are raised or lowered, and the slanting position of the stick makes it possible to tighten up the lianas independently of each other without the noose-snares getting mutually entangled. At the joint between the liana and the string there is suspended a signal bell, which immediately rings alarm when a bat has been caught in a snare and is struggling for all it is worth to liberate itself. A couple of these bells may be seen just below the bottom cross-piece. (*See also Fig. I.*)

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\* I also wish to take this opportunity to express my sense of indebtedness, in addition to the gentlemen already mentioned, to Mr. E. Olson-Manke, B.A., who has been kind enough to interview, on my account, Rev. Hammar on this matter.

The noose-snares, which are suspended close together on the lianas, are made of raphia fibre, *i.e.*, those fibres resembling the hair of the elephant's tail that are formed at the petiole bases.

The bells usually consist of the shells of large land-snails (*kodia*), or of the nuts of the fan-palm. The clapper is made of bone or hard wood. The bells of the different lianas are of different pitch, so that in the darkness it is possible to

distinguish, from the sound, on which of the lianas a catch has been effected. The person attending the trap keeps watch inside a small hut erected near by, and as soon as a bell sounds he hurries up to the trap and hauls down the catch, lest it should warn and frighten away other bats.

Trap arrangements of somewhat divergent form are represented in Plate F, Fig. 2.

When the position of the trees is suitable, the lianas are stretched directly between two trees. In such cases the bells may be tied on directly to the liana, one bell at either end of it (Plate F, Fig. 3).

FIG. 1.—ONE END OF A TRAPPING APPARATUS FOR EDIBLE BATS, SHOWING AN ALARM BELL (A SHELL), AND OUTSIDE OF IT THE ROPE BY MEANS OF WHICH THE WHOLE CONTRIVANCE IS FASTENED TO A TREE. MADZIA, BASUNDI, LOWER FRENCH CONGO. (ex LAMAN'S COLLECTIONS).

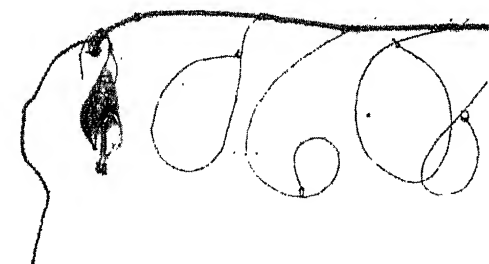


Plate F, Fig. 4, shows another way of arranging the noose-snares which, according to Laman, is of rarer occurrence than the type just described. The photograph, which was taken among the Bateke on the Congo river just below Brazzaville, speaks for itself. A row of snares arranged in this way is called *muta* (pl. *mila*). As will be noticed from the picture, there is only *one* line (of rattan), which, with this arrangement of the snares, of course is quite sufficient, as they will form something in the way of a net.

The animals that have been captured in this way are boiled or roasted. If the latter, they are skewered on a stick which is stuck into the ground and made to lean over the fire. When there has been an abundant catch, the surplus is brought to the market, made up into bunches of up to half a dozen animals.

As in other kinds of hunting, rites of magic import are performed here also, partly to ensure abundance of the catch, partly to protect against malignant influence. In this connection Laman informs me, in a letter, that it is the proper thing, at the beginning of each trapping season, to crush between the teeth the skull of the animal first caught, and then to eat the flesh. This is obviously an instance of the widespread conception of the importance of "first-fruits" (*cf.*, for example, the virtue of the bread first baked from the new harvest). Other magic acts are also performed, which, however, Laman was unable to recollect for the moment.

The large fruit-eating bats are so widely distributed over Central Africa that one is astonished in not, as it appears, finding the trapping arrangements here described also in other places. Is this because it is only the tribes on the Lower Congo who eat these animals? Or is there a lacuna in our knowledge? To these questions I am as yet unable to furnish a reply.

The only analogy to this trapping method that I have found in the literature originates from the same regions, and has been mentioned by Dr. P. Briart and by J. S. Jameson. At the village of Banza-Baka, Dr. Briart noted the following appliance for "bird-catching":—Two poles, from 8 to 10 metres high, and forked at the upper ends, were planted in the ground at a distance of 100–150 metres

from each other. They were connected by a long and strongly-made rope of lianas carefully joined together, on which intervals of 10-20 metres were marked out by a knot or a spike. These intervals serve to show the places where the snares were to be fastened, to the number of two or three at each node. The snares are thin and pliable, though very strong, and are made from vegetable fibre (generally from the raphia palm). "When the birds are caught in the snares, the line is loosened at one end, and let down so that the snared animal can be easily removed. In a number of suitable places about the village of Banza-Baka, Briart saw about 50 or 60 of these trapping apparatuses set up, containing, all told, some 40,000 snares."\* What kinds of birds these traps were intended for is not mentioned by Briart. Jameson made an observation in the neighbourhood of the junction of the Inkisi river with the Congo. He says: "The natives here have a curious method of catching birds by hanging long ropes, formed of creepers, from the trees on the edge of the forest to poles stuck up in the ground about 15 to 20 yards off. Hanging from these ropes are numbers of snares made from finer creepers, in which the birds are caught as they fly past."† Jameson illustrates his description with a sketch showing how, at the edge of a forest, out in the open and parallel with the trees, there are three poles planted in the ground. Each of these poles is connected by a rope with a corresponding forest tree. Most of the snares are single, but some occur in pairs, disposed in a manner similar to that seen in Plate F, Fig. 4.

The apparatuses described by Briart and Jameson are of about the same type as those described above and they too were presumably used in catching edible bats, not birds.

In conclusion, it might be added that the method of attaching signalling bells to trapping apparatus is, no doubt, known from other parts of Africa. The Shilluk of the White Nile tie a small bell made from the fruit of the doom palm on their spring-traps for antelopes and gazelles.‡ GERHARD LINDBLOM.

#### DESCRIPTION OF PLATES.

Fig. 1. Trapping apparatus for fruit-eating bats, *n'gembo*. Neighbourhood of Madzia, French Congo. (Hammar, photo).

Fig. 2. Snares for *n'gembo*, Lower French Congo.

Fig. 3. Apparatus for catching *n'gembo*, showing alarm bells and, below, hut for the hunter. (Hammar, photo.)

Fig. 4. Snare for *n'gembo*, Bateke, Lower French Congo. (Laman, photo.)

#### America, Central: Chronology.

Thompson.

##### Some New Dates from Pusilha. By J. Eric Thompson.

70

During the month of October the writer visited the ceremonial centre of Pusilha on behalf of the Field Museum, Chicago, and it is with the permission of that institution that this article is published.

This city is situated on the mahogany concession of Mr. Lee Pearce, of the Toledo settlement, British Honduras. The ruins are situated at the juncture of two tributaries of the Moho River, the Machaca or Pusilha River and the Juventud, a mile or so within the British side of the Guatemalan-British Honduras international frontier. Earlier in the year the writer had visited this site at the invitation of Mr. Pearce, but at that time the ruins were believed to occupy only the right

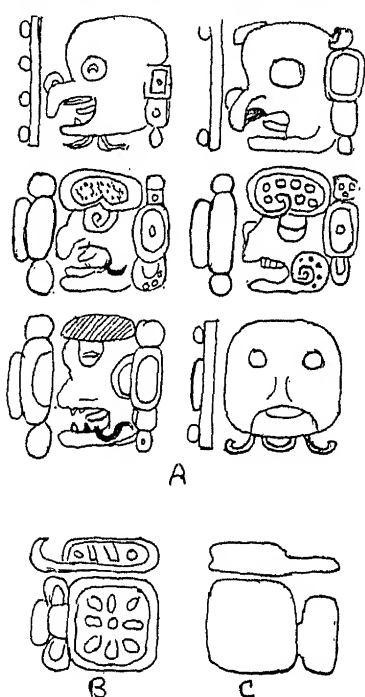
\* *Globus*, Band 70, p. 148, 1896. (From P. Briart, in "Le Mouvement Géographique," 1896).

† J. S. Jameson, "Story of the Rear Column of the Emin Pasha Relief Expedition," p. 23. London, 1890.

‡ W. Hofmayr, "Die Shilluk," p. 315. Mödling, 1925. On an alarm-bell against Civet-cats among the Pangwe (Fang), vide G. Tessmann, "Die Pangwe," p. 135. Berlin, 1913.

bank of the Pusilha River. A brief account of the ruins as then revealed will be published in the report of the British Museum's expedition to British Honduras during the 1927 season. Since that first visit further ruins and a group of stelæ were discovered by Mr. Mason while driving a tractor through the forest in the peninsula lying between these two rivers.

During a very hurried visit the writer was able to discover seven stelæ and one lintel. Six of the stelæ were lying in a fragmentary condition in an area of



not more than twenty yards square in front of a low mound, on the slopes of which were found the broken halves of the lintel. The other stela (No. 7) was found lying a hundred yards away. Both halves of the lintel (No. 1) were lying face downwards, and, with the exception of the Initial Series introducing glyph and the bottom right-hand glyph (B7), the inscription is in an excellent state of preservation. A3 to B5 give a straightforward 9-7-0-0-0 7 Ahau (see Fig. 1a). A6a glyph G of the Lunar Series; B6 glyphs E and D, the former with a coefficient of 5; A7 glyph C, with a coefficient of 6 (?), and glyph A with a coefficient of 10. B7 was presumably once the month date 3 Kankin, but unfortunately the surface has been chipped off by some blow and nothing remains. The glyphs show considerable signs of archaism and provincialism. The hand typical of the cycle glyph is missing. The Katun head variant bears more resemblance to that of the tun. The tun sign shows no sign of death, but is provided with a fang. The uinal glyph alone is normal with its closed eye and prominent curled fang. The kin

sign also is provided with a fang. The Lunar Series, giving 25 days from a new moon, is correct.

Stela 1, broken in two pieces, contains five rows of 11 glyphs, giving 52 glyphs and three extra spaces for the Initial Series introducing glyph.

The inscription, strangely enough, opens in C1 D2 with the introducing glyph, the centre piece of which is a bearded head. C3 9 cycles normal form, D3 12 katuns also normal forms. C4 0 tuns with the coefficient above and this sign (Fig. 1a) to the left. D4 is rather worn, but resembles the uinal glyph with a suffix that possibly represents the tun variant for zero. C5 is shown in Fig. 1b, a new variant of the kin glyph. There is no day sign in the next glyph block (D5), but what might be a variant of glyph C with a coefficient of 1. C6, C8, and D8 are glyphs C, B, and A of the Lunar Series, the first with a coefficient of 3, the last with a coefficient of 9. These follow six rather worn and apparently unknown glyphs, and in C11 and D11 clearly and unmistakably the calendar round date 10 Ahau 8 Yaxkin required to fit the Initial Series reading of 9-12-0-0-0. The reading presumably passes from D11 to A1, which is illegible. B1 is apparently 5 katuns. The remainder of the glyphs are illegible with the exception of B3, which is 1 Ahau enclosed on each side with a bracket. A4 3 with an obliterated sign beneath. B4 a clear 10 Ahau; A5 an obliterated sign with a rounded suffix. Column E was covered with a thick slime. Unfortunately, there were no means of cleaning it sufficiently well to allow of the secrets being wrested from any of these worn glyphs.

Stele 2 was also broken; the top half was recovered. This piece contains an introductory glyph followed by two rows of eight glyphs each.

A3-B4; 9 cycles, 3 Katuns, 0 Tuns, 0 Uinals with straightforward, though somewhat obliterated, normal period signs. A5 is apparently the kin glyph with what is probably the tail element on the right. The coefficient above is absent, or is represented by the irregular outlined suffix shown in Fig. 1c. B5 is probably ahau with a coefficient of one, two or three. It resembles more three, but if the kin coefficient is zero, the Ahau sign should have a coefficient of two. A6 is an unidentified sign with a coefficient of 12. There is no crescent between the two dots, which would make the reading of 3 as the coefficient of B5 more acceptable. B6 is also an unidentified sign with a coefficient of 4. A7a is also very worn; beneath it are two bars. A7b1 has a suffix of 3; B7 is glyph A of the Lunar Series with a coefficient of 10; A8 has a coefficient of 8, and A9 one of three. The two best readings are:—

9-3-0-0-0 2 Ahau 18 Muan. The month glyph in this case would be in the unrecovered fragment of the stele, or

9-3-0-0-14 3 12 Kayab. The 12 Kayab being glyph A6.

Neither of these dates are very satisfactory, and in any case neither is likely to be the contemporaneous date of the monument.

Stele 3 is represented by a fragment consisting of 4 columns of glyphs. A1 is the introductory glyph. B1 9 cycles with the head variant. Again no sign of the hand is visible. A2 is 14 katuns. B2 appears to be 0 tuns. The fracture passes diagonally across A3 and nothing is legible. B3 is illegible. The rest of these two columns is missing. Glyph C1 is a head with a coefficient of 1. Nothing else is legible. As the tun coefficient is apparently zero, the whole date is probably an even tun. The remaining stelæ could not be turned owing to their considerable weight. Stele 4, the largest of the lot, was slightly over 15 feet long. Stelæ 1, 2 and 3 have the unusual feature of a squared-off top.

The dates recovered, therefore, were:—

Lintel 1 -	-	9-7-0-0-0	7 Ahau	3 Kankin.
Stele 1 -	-	9-12-0-0-0	10 Ahau	8 Yaxkin.
Stele 2 -	-	9-3-0-0-0	2 Ahau	18 Muan?
Stele 3 -	-	9-14-0-0-0	6 Ahau	13 Muan?

Fig. 1. A Lintel 1 glyphs A3-B5. B the kin variant on stele 1. C the kin glyph of stele 2.

J. ERIC THOMPSON.

## Britain: Archæology.

Piggott.

### Excavation of an Early Iron Age Site at Knighton Hill, near the White Horse Hill, Berks. *By Stuart Piggott.*

71

About half a mile west of the earthwork of Uffington Castle a rough road runs up the north slope of the downs to the Ridgeway from the little village of Compton Beauchamp. On the west of this road, which is marked on the Ordnance map as Knighton Hill, at a point just before it reaches the crest, is a large chalk pit dug to a depth of about ten feet into the hillside. The section on the western face of this digging showed that it had cut through a series of pits (showing in section) of varying sizes and shapes. The surface of the ground as yet untouched is ploughed, and shows no sign of any hollows or depressions marking the sites of further pits, nor is there any pottery strewn about. These pits showed an unstratified filling of mould and chalk rubble, with pieces of rough pottery, bones and charcoal.

In August, 1926, I measured and plotted the pits as then existing, and a preliminary (unillustrated) report on the site was published in the *Berks., Bucks.*



and *Oxon. Archaeological Journal*, Vol. XXXI, Part I. and in *Antiq. Journ.*, Oct. 1927. In January, 1927, I again visited the site and found that since my last visit chalk digging had destroyed one pit, and threatened three others. Consequently it was decided to clear out the filling carefully from the pits, and to collect the pottery and

other relics. The work was carried out by my father and myself, with the help of one labourer who was working at the pit. Photographs were taken of the pits as then existing. A further visit in the summer of 1927 was made, but all the pits (1-4) had been destroyed.

*The Preliminary Survey.*—In the drawing, Fig. 1, I have shown the positions and sections of the pits as seen in August, 1926. The numbering is that adopted in the report, although Pit 1 was destroyed before January, 1927. Pits 5 and 6 have not been excavated.

*The Pits.*—The pits excavated were of two main types: deep well-cut (3 and 4), and shallow irregular hollows (1 and 2). The fillings in all cases consisted of unstratified mould and chalk, mixed with small pieces of "sarsen" stone and in some cases containing quite large chalk lumps. Bones

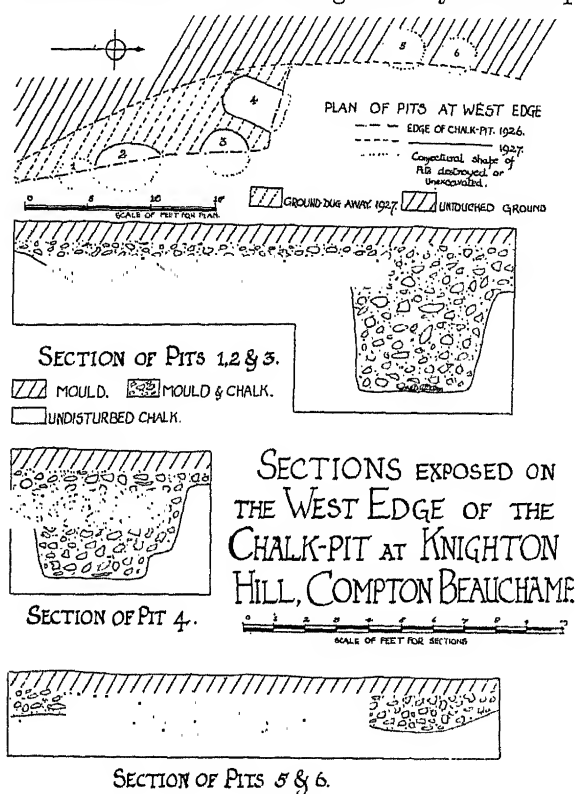


FIG. 1.

were found at all levels, but the pottery tended to be more abundant near the bottom. Charcoal was common in the filling, and at the bottom of Pit 3 were found traces of a hearth. For details of pits, see the table at the end.

#### OBJECTS FOUND IN THE PITS.

*A. Iron.*—One object of iron was found at 18 inches in Pit 4. It has been made from a flat bar, about  $\frac{1}{2} \times \frac{1}{4}$  inches, with the ends flattened out and apparently originally perforated. Use unknown, unless it is a small bridle-bit. (Fig. 2, 1).

*B. Spindle-Whorl.*—A spindle-whorl of local stone (used at the present day for roofing slabs), in the form of a flattened sphere, well finished. It has had a large piece chipped from it in ancient times, but the perforation of uniform bore is intact. Found in the face of an otherwise unexplored pit (Pit "x") some 15 yards from the main group (Fig. 2, 2).

*C. Objects of Bone, Figs. 2, 4-9.*—Several pieces of bone were found split as if for use in some fashion. One bone shows two long knife-cuts, and another small splinter has been cut into shape. No definite bone or antler tools were found.

*D. Boiling Stones.*—It is remarkable that only two boiling stones were found—in Pit 4 at 3 feet. It appears that the cooking was done by boiling in the usual way on a hearth, and this is borne out by the fact that nearly all the sherds

presumably belonging to the lower part of vessels were, when found, thickly coated with soot.

*E. The Hearth.*—Resting on the flat bottom of Pit 3, and nearly in the middle, a number of pieces of burnt "sarsen" stones were found, in association with much charcoal. This seems to have been a hearth site. Part of the upper jaw and the incisor of a small sheep were found near.

*F. Burnt Clay other than Pottery.*—A small piece of burnt clay, with two smooth faces, was found in Pit 4. It may possibly have been a piece of clay daub forced into an angle and so retaining the shape. (Plate I, Fig. 3).

*G. Mollusca.*—No marine shells were found, and only one land species—*Helix nemoralis*—was found in Pit 4 at 10 inches.

*H. Wood Charcoal.*—The wood charcoal found in abundance in Pit 4 was submitted to the Director of the Botanical Gardens at Kew, who kindly examined it and reported that in one case the wood had been identified as alder buckthorn (*Rhamnus frangula* L.)

*J. The Pottery.*—Mr. Reginald Smith, V.P.S.A., has very kindly examined the greater part of the pottery and his identifications have been invaluable.

With the exception of the base of a large vessel of black pottery (Fig. 13), which appears to be Anglo-Saxon, about 6th century A.D., and which was found at the top of Pit 4, the whole of the remaining pottery Mr. Reginald Smith assigns to the late Hallstatt and early La Tène periods. It has not been possible to date individual pits, as both types of pottery (soapy and polished La Tène and rough light brown hard ware, presumably late Hallstatt) occur together in all pits.

In view of the fact that no Romano-British ware was found at Knighton Hill, the presence of the one piece of Anglo-Saxon pottery is remarkable, especially as no Saxon remains of any kind have been found in kindred villages of the Late Celtic period. The presence of one piece of pottery, however, cannot be taken as proof of continued occupation of the site into Saxon times. The White Horse Hill district is fairly productive of Saxon remains, and several Saxon interments were excavated by Martin Atkins in the 19th century near the White Horse itself. The piece of Saxon ware, then, may quite reasonably be an accidental intrusion after the village was deserted.

#### DETAILS OF THE POTTERY. FIGS. 10-20.

Fig. 10.—Piece of upper part of pot. Paste grey, hard. Exterior smooth, brownish-red and black. Hand-made. From Pit 4.

Fig. 11.—Piece of upper part of large vessel (cooking-pot?). Coarse hard ware with large flint grits in the substance. Hand-made. The most abundant type of ware, found in quantities in all pits. Exactly similar rim fragments from Pits 1 and 4. From Pit 2.

Fig. 12.—Hard reddish-brown hand-made ware with occasional flint grits. The largest joined piece found. From Pit 2.

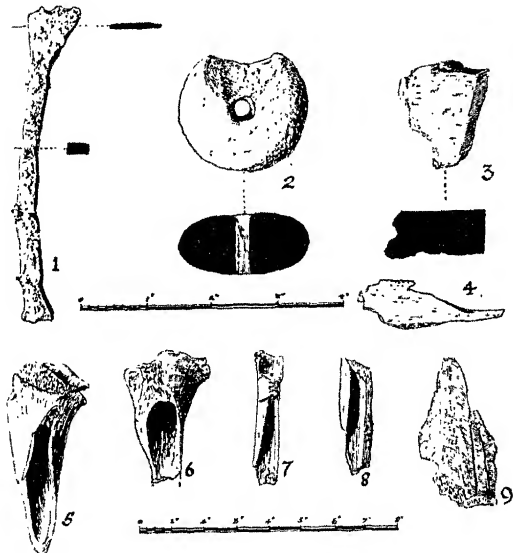


FIG. 2.

Fig. 13.—Hard grey ware with black burnished exterior. Assigned by Mr. Reginald Smith to 6th century A.D. From top of Pit 4.

Fig. 14.—Hard grey paste, exterior red to black. Hand-made, and part of shoulder of vessel, ornamented with "finger-tip" design. The only decorated piece found. From Pit 3.

Fig. 15.—Grey paste with black burnished exterior, probably part of the side of a vessel near the base. There were originally two perforations, across which

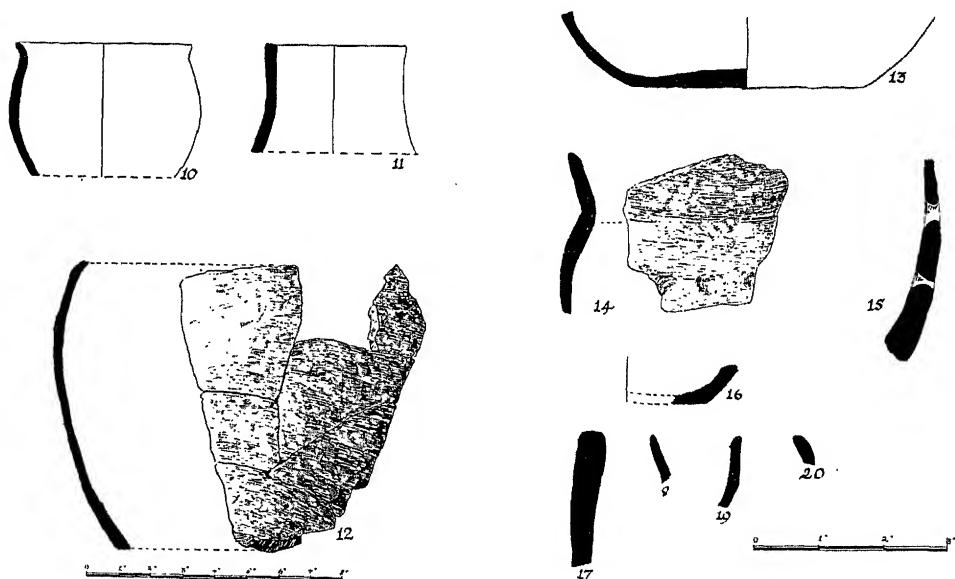


FIG. 3.

the sherd is broken, and the drawing shows the section through these holes. From Pit 4.

Fig. 16.—Grey paste with black burnished exterior, the only Celtic base fragment found. From Pit 3.

Fig. 17.—Rim fragment of grey paste with smooth grey surface. Pit 3.

Fig. 18.—Rim fragment of grey paste with reddish exterior. Wheel-turned. From Pit 3, near hearth.

Fig. 19.—Rim fragment of grey paste. Wheel-turned. From Pit 4.

Fig. 20.—Rim fragment of similar wheel-turned ware. From Pit 4.

Interiors to right in rim-section drawings.

#### POTTERY TYPES NOT ILLUSTRATED.

21.—Light greyish ware mottled with darker grey. One small piece was found in Pit 4.

22.—Burnished ware (hæmatite-coated?) of pinkish brown colour. Small piece from Pit 3.

23.—Soft ware red on outside and black on inside. Both surfaces are very smooth. One piece from Pit 3.

#### CONCLUSIONS.

From the material afforded from the four pits explored, we can form a fairly clear idea of the settlement at Knighton Hill. The pits themselves do not seem to have been used, at any rate as a general rule, as dwellings; indeed their smallness prohibits this: the presence of a hearth site in Pit 3 suggests, however,

that they were not only used as storage or refuse pits. Their original purpose, however, was probably for storing grain or roots, under the huts, and later for rubbish (bones, broken pottery and the like). The pits have been filled artificially, not by natural silting.

Doubtless the pits excavated belong to a much larger group, which may either have been dug away or are still to be unearthed. It is hoped that in the near future it will be possible to determine if the latter view is correct, by running a trial trench westward from the edge of the chalkpit. It seems that some smaller pits must have been destroyed, for the workmen at the pit remember finding pottery and bones for quite a long time, but expressed surprise at the two larger pits (3 and 4) which they said were unlike, and much larger than any they had previously dug through.

In the absence of any finds which may be accurately dated, it seems that the site was inhabited in the middle of the early Iron Age: in late Hallstatt and early La Tène times, and putting aside the one piece of Saxon pottery, that it did not continue in occupation until Roman times.

TABLE OF PITS.

Pit.	Depth.	Width.	Length.	Shape.	Orient- ation	Finds and Remarks.
1	20"	46"	Not known.	Circ. (?)	—	Destroyed before January 1927. Pottery and bones. Half upper jaw, boar; two lower jaws, sheep. Pieces of cooking pot.
2	36"	78"	Not known.	Oval	N.	The length of all pits is unknown because of the digging which destroyed one side. Bones and pottery. Six pieces of large vessel. Skull of horse, skull of goat. Bones of sheep, etc.
3	60"	54" (top) 44" (base)	50"-40" (?)	Circ.	—	Bones and pottery. Polished ware. Hearth and charcoal at bottom.
4	50"	60" (top) 36" (base)	70"-80" (?)	Sub- rect.	N.E.	Bones and pottery. Charcoal, Anglo-Saxon pottery at top. Iron object. Skull of horse. Largest pit excavated. Shelf on N. side.
5	18"	50"	—	—	—	NOT EXCAVATED. Pottery and bones from face.
6	18"	40"	—	—	—	Do. Do.
"X"	—	—	—	—	—	Traces of pit about 15 yards from main group. Rough sherds and spindle-whorl.

Archæology.

Abbott.

**The Necessary Qualifications for the Study of Comparative  
Flint-Flaking.** By W. J. Lewis Abbott, F.G.S.

72

I yield to no one in my admiration of some of the excellent field work of Mr. Hazzledine Warren: his splendid Glacial work in the Lea Valley was just what was wanted to fit in with the *Betula nana* bed at the Admiralty, and other sections, showing the pre-glacial age of the Valley of the Thames (and therefore pre-glacial Man); and also with the extended work over the S.E. England, which has culminated in the discovery of what appears to be a Glacial Moraine at Hastings with typical Mousterian implements, above and below the White Chalky

Boulder Marl, against which Mr. Warren now directs one of his annihilating blasts!

But Mr. Warren on paper is quite another matter! It is here he has an insuperable advantage. In a single paper he can deny a dozen of the best attested facts, or make a dozen false assertions in as many sentences; but to prove, or disprove, them might require as many pages!

In the evening of life, when the night-clouds are becoming more obtrusive, and one casts their eye along the trenches, and sees their comrades shooting wildly, handling high explosives dangerously, throwing them hither and thither, sometimes directing them point blank at one's self; and then realises how few there are in the trenches, and how hard and protracted the battle has been, how little has been achieved and how much there is to conquer, one is inclined to forget retribution, and to think more of how the unfortunate circumstances can be remedied, and the results of the experience of sixty years of fighting, observation, and experiment can be handed on to their comrades.

It seems to me that little good would result from contradicting claims and statements unjustifiably made, every time they appear, grievous and disappointing as they often are, and we ask: Has not the time come when we can be made to realise what are the laws and conditions underlying lithoclassiology? We all want to see the subject put upon a scientific basis, and yet how many can be made to realise what these laws are? A man sees some feature for the first time upon a flint he believes to have been worked by man, and forthwith he makes this the hall-mark of man's authenticity, whereas were he to walk for a few hours along a beach, or study other forces in nature, he would see his gigantic error. The converse is also true, he sees a feature upon a flint he believes to have had a natural origin and forthwith every specimen presenting anything approaching this feature is relegated to the realms of nature.

Unfortunately we are mostly collectors, or readers and writers, we do not start at the beginning. Sir John Evans spent his last observing weeks studying the immense collection I have got together in sixty years: to illustrate lithoclassiology. His last words to me were: "Promise me that, if I do not pull through the operation, that you will publish all this: it is where we ought to have begun sixty years ago, and it is here we must re-begin to-day." Instead of beginning at the beginning men are for ever finding these new hall-marks of nature or man, of so absurd a character that one cannot help feeling grieved. So intent are they upon establishing their opposition to the conclusions of others, that they will not stop short at deliberate mis-quotations! or they will challenge the honour of their life-long friends, whom they know to be of unimpeachable character!

For many years Mr. Hazzledine Warren spent his energy in seeking opportunities to oppose the so-called "Eoliths" (by which we will mean the pre-palæolithic worked flints of the plateaux). I respectfully submit that that gentleman has never seen a really good typical set of these, and therefore is not in a position to either appreciate or condemn them. For very many years they had no greater opponent than the late Sir John Evans, and a large number of people voted against them simply because Sir John did. Unfortunately, there is a great excuse for this, in the way over-zealous collectors mixed up the sheep and the goats. When, however, Sir John saw my collection, he demanded: "How is it I have never seen these things before? A man would be mad to oppose them"; and had he lived he would have lost no time in bringing the question before the public. In France they probably had no greater opponent than the Abbé Breuil. Now, by a remarkable coincidence, standing on the same spot, before a type case in my dining-room, when the conscientious Abbé saw the really fine series, he exclaimed, "Why have I not seen these before? *très joli! très joli!*"

I have carefully read all that came my way of what Mr. Warren has published about flint fracture, and I am prepared to show that he has not yet grasped the underlying elementary facts, that many of his assertions would be better appreciated from the pen of Gilbert and Sullivan—they are nothing more than burlesques, parodies and travesties: one has only to cite the terrible nonsense he recently published about the angle of the *écaillage* being the hall-mark of man, for this to be realised. He does not appear to realise in the least the origin of *écaillures*; he never refers to more than one kind, whereas there are obviously three; one due to the overlapping of the surfaces of rotation—these we may call surface *écaillures*, which leave an *écaillage* flake or plate, that may be of paper thinness and of only a square c.m. in dimension, or they may measure a hundred cubic c.m. and attain a thickness of two c.m. and upwards, leaving a corresponding scar upon both core and flake, to which we generally refer as an *écaillage*. The second are due to hetero-refragability, and may be of any number and shape and in any direction; and the third to rebound, or second striking, which will be more or less in the striking plane and ripple normal to it. It is obvious that man has nothing to do with them, or they with man. The Belle Asses things were known and studied in this country long before the interesting specimens were found on the Continent. The operation is of a most selective nature, and differs in every possible way from the foregoing, and can only operate at all in substances of very low refragability index and in the most colloid and isoclastic of flints, and is not the result of percussion, but of pressure. I respectfully submit that, seeing Mr. Warren has never even seen a good typical set of plateau “Eoliths,” and has not grasped the laws underlying the production of large flakes, his condemnation is as worthless as his appreciation would be!

I regret to have to say that his statements about the large flakes of the Cromer Forest Bed things are as false as they are ridiculous, and I am very sorry space will not allow me to show this in this article; but I should be pleased to do so on another occasion, and also that if there is one thing possible, it is to show the evolution of flaking—“comparative flaking,” if we prefer that term. But, as it requires a palæontologist to read “the record of the rocks,” so it requires a lithoclassiologist to read “the record of the flints.”

W. J. LEWIS ABBOTT.

## REVIEWS.

Technology: House-Types. Oelmann.

*Haus und Hof im Altertum. I. Die Grundformen des Hausbaus.* By **73** Franz Oelmann. Berlin und Leipzig (De Gruyter), 1927. Large 4to (14 × 10 in.). Pp. viii, 132. With map and 85 illustrations.

This handsome volume is the introduction to a series of regional studies of the principal types of domestic buildings in antiquity. But its scope is wider, and the author's account of his enquiries is interesting. It all began with his difficulty in interpreting the ground plan of a Roman villa at Blankenheim in the Eifel, and his realisation that such difficulties mainly result from uncertainty as to the purpose of the various parts of the building, and consequently as to the builder's design. Most obscure of all, in any ruinous building, is the construction of the roof; for all buildings are, at long last, devices for

protection against the weather. The weather, in turn, has several components: A roof may keep off the sun well enough, but be futile in rain, and dangerous in a wind. Fortunately, the weather also varies regionally; and house types which vary regionally result either from regional peculiarities of the weather, or from the intrusion into a region with one kind of weather, of people accustomed to build houses to suit another kind somewhere else. Obviously, a further “control” is exercised by presence or absence of building materials—above all, of timber—and this, too, depends on rainfall. Applying these general notions, and his very wide acquaintance with the building constructions of all peoples, with great ingenuity, and admirable restraint in the use of his imagination, Dr. Oelmann has produced, for the first time, a rationalised account of one of the most fundamental of the arts; and has

thrown most valuable light on the distribution of house-types, which is indeed exceedingly puzzling; but is certainly much more easy to understand with the help of this analysis.

Not the least valuable aspect of this enquiry is that it provides the material for a fresh examination of current theories of the diffusion of cultures. The author confesses himself a disciple of Gräbner and Schmidt, but he is too good a geographer to let ethnological theory run away with him. His criticism, for example, of Noack's identification of the "narrow-fronted" *megaron* of Mycenæan Greece with the gable-roofed types of the "cold north," is that "narrow-fronted" gable-houses are characteristic also of tropical forest regions: it is not the temperature but the rainfall which is the relevant factor in the geographical distribution, because it determines the form of the roof, and thereby not merely the form of the building but its relation with any neighbouring buildings: there must be free ground under its eaves so that the drippings may not give trouble. This simple restatement of essentials at once clarifies Noack's own argument, explains other peculiarities of the Mycenæan *megaron*, and opens fresh prospect of discovering the source of this intrusive feature in Ægean architecture.

On the other hand, where he sees reason to be bold, Dr. Oelmann does not hesitate. A striking example is his belief that certain house-forms of West Africa and Indonesia are really akin, based on the fact that the cultures to which they respectively belong both have the cultivated banana. There was then once a distribution of climates in intervening regions which permitted the propagation of this plant from some common source, among people of a quite low stage of advancement.

Not only professional architects, and specialists in the technology of dwellings, but all ethnographers and archaeologists, will find this book well worth their attention. It is not only of considerable scientific importance, but an admirable exposition of method, and (may we add) most readable; the illustrations are excellent; and the bibliographical notes testify both to Dr. Oelmann's wide reading, and to his keen and constructive criticism of his predecessors.

J. L. MYRES.

#### Economics.

Burns.

*Money and Monetary Policy in Early Times.* By A. R. Burns, B.Sc. 74 (Econ.). Kegan Paul (History of Civilisation). Pp. xiii. + 517. 16 plates and 1 map. Price 25s. net.

Economists have had the privilege of seeing their subject holding out longer

than most others from the historical criticism of the anthropologist. But their immunity is nearing its end, and it seems that the difficulties of modern nations will increasingly encourage, if not demand, that the human significance of economic institutions shall be disentangled from the theories which belong to their historical accidents. The most baffling of these institutions is money. It has had many historians, but its sociology remains to be written.

The book before us is another descriptive treatise and is concerned mainly with the institution of coinage and its problems in the ancient world from its beginnings in the Eastern Mediterranean to the reforms of the Roman coinage by Constantine in the fourth century A.D. The numismatist, the historian, as well as the sociologist of money, will find here the evidence which has accumulated on the subject during the last century set forth historically and analytically in a thorough and workmanlike manner. Mr. Burns, whose industry has been enormous, and whose imagination is critically restrained where gaps in the evidence cannot be left nakedly open, has used the accumulating archaeological and literary material to extend and develop the treatment of problems which Sir William Ridgeway opened up in his "Origin of Currency and Weight Standards."

After an opening chapter in which he exposes the arbitrary distinction between a barter and a money system of exchange, the author devotes the greater part of his work to a description of the beginnings of coinage and the practical difficulties of its administration. The types, the material and fineness of early coins, weight standards as the precursors of a guaranteed coinage, the nature of the prerogative of coin issues and the difficulties of providing subdivisions of the recognised units of value, are set forth and critically examined from the point of view of a modern orthodox economist. It is mainly, however, the remaining chapters which render the book an important and significant new work. The motives which dominate monetary policy in early times are deduced or inferred from the actual changes in coin issues, and a comparison made between these policies in Lydia, Persia, Macedon, Egypt and China, in the Greek World, in the Roman Republic and Empire. These chapters constitute an enquiry into the relation between the various issues of monetary units and changes in the value of money, with their economic and political consequences.

Many questions are raised which, owing to lack of evidence, cannot be answered,

but there are a number of clues which should be of interest to the sociological economist. For one thing, the diffusionist will have no contradiction in claiming the institution of coinage as one which has spread from a single beginning. There seemed no natural desire for it in the ancient world. In fact, its gradual spread had many resistances, notably in Sparta and China. Phœnicia, Mesopotamia and Carthage held out a long time before realising its advantages, and in Egypt payment in metals by weight was adhered to as long as possible. The reflection of religious and political influences on coin types is emphasised, especially as illustrating the differences between the monarchies of Persia, Macedon, Rome, and the more democratic states of the Ægean and early Italy. Gold, as the standard metal of the currency, seems to have an interesting association with imperial rule. One of the most striking impressions left by this survey is the secondary part in financial policy often played by the true economic motive of facilitating exchange. For example, Mr. Burns suggests that in the West coinage largely became popular because it provided an incentive to the export of the precious metals. Professor P. N. Ure, in "The Origin of Tyranny," has already made us familiar with the power of currency manipulation in acquiring and maintaining rulership, and throughout the reading of Mr. Burns's book we are struck with the subservience of economic to other motives. The display of civic pride and independence played almost as great a part in the variation of coin types in the Greek world as the desire to simplify trade for its own sake, and in the Asiatic Empires coinage seems largely to have developed because of its psychological value to the monarchy. During the Roman Republic moneyers used the coinage for swaggering family advertisement, and the Republic was followed by the determined depreciation of the currency by the Emperors in the interests of profit to the state.

Mr. Burns hopes that his work may supply the means of "checking our monetary policy." To the present reviewer, as a social anthropologist, the instability of any historical currency and the difficulties of states in times of emergency such as the Republic experienced during the Punic Wars and modern nations during the last decade, suggest the impossibility of devising a permanent medium of exchange and unit of value which is at the same time a commodity, whether it be gold or any other thing which is desired apart from its exchange properties. Token money, to which Mr. Burns devotes a

rather derogatory chapter, may turn out to be the most intelligent step taken by man since the invention of money itself.

The author has given us an extremely useful treatise on an important chapter in the history of this most elusive of our institutions—and perhaps the one most uncritically tied to the conditions of its beginnings.

V. A. DEMANT.

#### Ethnology.

Parsons.

*The Earlier Inhabitants of London.*

By Professor F. G. Parsons. **75**  
London: Cecil Palmer. 1927. 8vo.  
Pp. 240. Price 10s. 6d. net.

Professor Parsons' valuable work on the craniology of the early occupants of these islands, and of the skulls and bones from later ossuaries, plague pits and the like, is well known to all fellows of the Institute; as also are his studies on the coloration of modern English. In the present work he sums up the results set out in detail in various papers he has read before the Institute and the British Association, incorporating with the less technical details a mass of information on the early history and archæology of London and the south-eastern counties. The result will be welcomed by all interested in early history, and especially by those who might find the original papers rather over-full of figures and measurements but who can fully appreciate a popular summary of the results. Professor Parsons indicates the multiplicity of the sources whence our population has been derived and the reasonable certainty of the survival of traits derived from all these sources. The predominant element is unquestionably Nordic, though some Neolithic strains survive, and in the last thousand years there has no doubt been further reinforcements of Mediterranean and Alpine blood, the latter in particular seeming to have increased in recent years. Professor Parsons gives a full account, with illustrations, of the main types of occupants of the island, some of whom, such as the beaker folk and the "prospecter," he thinks came in only small numbers and were migrant traders rather than permanent settlers. The general character of our people he regards as essentially Nordic, though in some measure modified and softened by improved cultural opportunities.

The history of London itself cannot be traced with any certainty prior to Roman times, though the presence of trackways point to the use of the ford at Westminster from Neolithic times.

The author agrees with Professor Haverfield that London seems to have sprung to size and prosperity owing to the influence



of Roman traders and its uniquely convenient position.

It will be seen that the present work is one which could serve to popularise the use of anatomical data to supplement history and archaeology, and that its form is one to render most useful service to the general reader and student.

F. C. SHRUBSALL.

Papua: Ethnography. Armstrong. *Rossel Island: an Ethnological Study*. By W. E. Armstrong. With an Introduction by A. C. Haddon. 8½ in. × 5½ in.; pp. xxviii, 274; 34 plates and diagrams and a sketch-map. Cambridge: At the University Press, 1928. Price 18s. net.

Rossel is the most easterly island of the Louisiade group; about 1,500 persons live on its 100 square miles; and its isolation, together with the evil cannibalistic reputation of its inhabitants, have caused it to remain almost virgin ground for the anthropologist. On this island Mr. Armstrong spent about two months in 1921, and in the present volume he reports the work then done. In plan and execution his book is excellent, for to his general ethnological account he has added a valuable introduction by Dr. Haddon in which the physical anthropology and the cultural significance of the area are discussed; a careful history and bibliography of the island, in which is recorded everything anthropological previously published about it; physical measurements of 26 Rossel natives; a general theory of the classificatory system (in addition to a good deal of theory by the way); and a good index (but why were the important appendixes not indexed?). In short, so far as it goes, Mr. Armstrong's book deserves to be described as a model ethnological study. But it is no use pretending that the author has given us a complete, or even a thoroughly reliable, picture of Rossel. Two months cannot suffice for that, be the anthropologist never so expert an observer. In Mr. Armstrong's account such phrases as "according to so-and-so," "I failed to ascertain," "I did not fully elucidate," occur far too often, though, of course, they are tributes to his conscientiousness and accuracy.

Mr. Armstrong has at least two interesting discoveries to his credit. One is of a novel system of currency, which has already received a good deal of notice, but which does not appear to be much more than a freak, of little general significance. The most important thing brought out in this connection seems to be the fact that the natives of Rossel can count up to 10,000, "or even further," unlike those

of Massim, who have a laborious system of notation, largely based on 5 as a unit, in which anything above 20 is difficult of attainment. This monetary system operates with two kinds of money: *ndap*, which consists of ground-down and polished pieces of *Spondylus* shell, and *nkö*, the units of which are sets of ten discs of shell, probably made of a giant clam, perforated and strung together. *Ndap* money has 22 main intrinsic values, and another series of values in relation to the object desired to be bought; that is, specific objects can only be bought with pieces of a specific value, and not with pieces of other values made up to the same total value. If a man does not possess a piece of the appropriate value, he borrows one, and makes his repayment with another piece higher in value in proportion to the length of the period of the loan. *Nkö* money, except that it has only 16 intrinsic values, is similarly used.

The second discovery mentioned above, if a true discovery it is, cannot easily be summarised without discussing Mr. Armstrong's classificatory theories in general. Space will not permit me to do so here, but I hope to execute this task in another place. Briefly, then, Mr. Armstrong describes a grouping consisting of a sequence of persons in the order of closeness of their relationship, this order being, naming only the closest, father, mother, brothers and sisters, children, wife or husband, mother's brothers and sisters, sister's children. If this sequence has been correctly observed, it should throw a great deal of light on the whole series of problems connected with the family and the tribe.

THEODORE BESTERMAN.

History. Bury: Cook: Adcock.

*The Cambridge Ancient History*. Edited by J. B. Bury, S. A. Cook, F. E. Adcock. (IV. *The Persian Empire and the West*; V. *Athens, 478-401 B.C.*; VI. *Macedon, 401-301 B.C.* Volume of Plates I prepared by C. T. Seltman). 9½ in. × 6½ in.; pp. xxiii, 698, 11 maps and plans and 6 tables + xxii, 554, 12 maps and plans and 2 tables + xxiii, 648, 9 maps and plans and 1 table + xxviii, 395, numerous ill. Cambridge: At the University Press, 1926-1927. Prices, 35s., 21s., 30s., 25s. net.

The previous volumes of this series were reviewed in MAN, 1924, 91, and 1926, 136. As the narrative proceeds onward, it becomes more and more purely historical, until in the present volumes there is only a small proportion of matter of direct interest to the anthropologist. Even so, he will find much of importance to his studies, to select a few instances only, in

F. E. Adcock on the blood feud (iv. 29 *sqq.*), in G. F. Hill on the history of coinage (iv, chap. V), in G. B. Gray and M. Cary on Zoroastrianism (iv. 205 *sqq.*), in R. S. Conway and S. Casson on Etruscan custom and religion (iv, 411 *sqq.*), in R. W. Macan's note on the anthropological aspects of history (with particular reference to Herodotus and Thucydides, v, 405), and in S. A. Cook on the inauguration of Judaism (vi, chap. VII).

The volume of plates, however, is a very different matter. These plates illustrate the first four volumes of the *History*, and

form a very striking pictorial conspectus of the history of man to the beginning of Greek civilisation. Professor Myres's essays in the first volume, for instance, are illustrated by reproductions of the earliest skulls, of palæolithic tools and art, and of neolithic implements, art, industries and pottery. But it would be impossible to describe the whole of the contents of these plates. The anthropological student who does not neglect any means of simplifying his researches, will not neglect the *Cambridge Ancient History's* first volume of plates. THEODORE BESTERMAN.

## CORRESPONDENCE.

### Ethnology

Hornell.

### Indonesian Culture in East Africa.

To the Editor of MAN.

SIR.—In MAN, 1928, 41, Dr. Elliot 78 Smith, while admitting the accuracy of my criticism of his description of the Victoria Nyanza canoes, claims that the correction strengthens rather than weakens his interpretation of the problem; he maintains that this peculiar canoe form from East Central Africa, differing fundamentally from any other existing canoe or boat type in Africa, is to be traced directly to an Egyptian origin equally with the type depicted in very ancient Scandinavian petroglyphs. In spite of his argument, I remain convinced that the Victoria Nyanza canoe owes its peculiarities of construction to Indonesian influence exercised through ancient Indonesian settlements upon the east coast of Africa. On the other hand, I am by no means desirous of denying that the Indonesian form of canoe, from which the Victoria Nyanza type is derived, has not had cultural relation with those forms graven upon Scandinavian rocks. I have noted a considerable number of peculiarities in the construction of certain types of vessels in Indonesia which have their counterparts in ancient Scandinavian naval architecture—peculiarities which, so far as I know, are unknown in normal and typical Egyptian ship construction.

The most notable of these are:—

- (1) The bifid prow;
- (2) The fitting in of the ribs subsequent to the putting together of the planks forming the hull;
- (3) The tying of these ribs by means of twine or thongs to cleats left on the inner side of the planks when they were adzed out.
- (4) The sewing of the planks together at the edges with fibre or cord.

(5) The use of a wooden bailer having the handle *within* the scoop, instead of projecting on the outside.

The bifid prow is known in Scandinavia only from very ancient petroglyphs, but the sewing of planks together with twine, and the peculiar fitting of the ribs to the planking, are features both present in a length of plank, probably the oldest fragment of a planked boat in existence, now in the Bergen Museum, and described and figured by Professor Shetelig of that Institution. The bailer design is even now occasionally met with on the Norwegian coast, and many examples are to be seen in Norwegian museums.

All of these peculiarities are met with in the Indonesian area, and several are found sporadically in Melanesia and Polynesia.

Similarities so peculiar and numerous cannot possibly be of independent invention, so we may take it as certain that these features, seen in ancient Scandinavian seacraft, and in present-day Indonesian vessels, are derived from a common source, but I cannot see how that can have been Egyptian.

While on the subject of the presence in East Africa of vestiges of Indonesian colonisation and influence, I would like to call attention to the suggestion made several years ago by Monsieur G. Ferrand, a great Malagasy scholar, in a paper published by the Société Asiatique, of the possibility of the ruins of Zimbabwe being the remains of a fortress built by Indonesian colonists and gold-seekers from Madagascar. This is a hypothesis which has never been examined in detail, and which certainly deserves to receive careful investigation; this, however, can only be done on the spot by someone thoroughly conversant with ancient Indonesian architecture. In this connection, I invite

attention to the similarity of the openwork ornamentation seen in the Zimbabwe walls to that characterising the construction of the numerous small bell-shaped shrines surrounding the great dagoba on the topmost platform of the Boro Budur temple in Java.

By a curious coincidence, this idea of the Malagasy origin of Zimbabwe has also occurred to that acute observer, Aloysius Horn,\* but I do not adduce this fact as having any evidential importance.

JAMES HORNELL.

Freetown, Sierra Leone.

21st April, 1928.

*To the Editor of MAN.*

SIR,—In view of the fact that the old controversy between diffusion and independent invention is being discussed in the columns of MAN with fresh vigour, I venture to analyse one of Mr. Hutton's examples "of identical results from different origins," hoping that I can slightly clarify the issue.

Mr. Hutton says (MAN, 1927, 5): "The poisonous property of adrenalin was discovered by physiologists investigating the endocrine secretions of the human body, and then artificially produced by chemists. It was independently discovered by Indians of the Amazon who use the adrenalin secretion of the glands of a certain toad as an arrow poison." The Indians of the Amazon basin had to learn that death can be caused by coming into contact with the adrenalin secretion of the glands of the toad in question; a sequence of events

which certainly did not *depend* on any brain work on the part of the victim (or onlookers). Such sequence of events causing death might occur independently anywhere where life comes in contact with adrenalin. To call this alone independent invention would be just like considering the drowning of a man in the Thames and the Amazon respectively as such. The intentional application of poison, on the other hand, is an invention involving some—though not much—thought by the individual. Of the various kinds of poisons, the one which is most easily accessible will be automatically employed once the art (too good a word!) of poisoning is possessed. We cannot hope to make conclusions about this subject until data relating to the first invention of the art of killing by applying venom is available. I think it is essential to the solution of the problem of invention to classify our concepts and define what exactly we mean by the phenomena. I think Mr. Perry would only call a procedure which involves mental action of a specific yet unsolved kind invention. Animals possess a capacity to imitate, but two monkeys copying the same action are certainly not the authors of an invention. Mr. Perry (MAN, 1927, 147) asks for "a real instance," by which I presume he means an instance of an "invention" apart from simulation. He cannot possibly doubt that different causes may give rise to identical results: such as the drowning of two persons "independently."

Yours faithfully,

GEORGE F. POWELL.

56, rue de Richelieu, Paris.

3rd May, 1928.

\* "The Life and Works of Aloysius Horn. I. The Ivory Coast." London, 1927.

## ANTHROPOLOGICAL NOTES.

*Honour for Dr. Marett.*—Dr. R. R. 79 Marett, of Oxford, has been elected a Fellow of the Royal Society of Letters of the University of Lund (Sweden). Only fifteen foreign members are eligible in the Philosophico-Historical Section, and Dr. Marett is, therefore, to be congratulated on a distinct honour. The University of Lund, it may be recalled, has produced several anthropologists of note, among them Professor M. P. Nilsson, author of "Primitive Time-Reckoning," who is the Secretary of the Society.

*Exhibition of Flint Implements from East Anglia.*—By kind permission of the Society of Antiquaries, an exhibition is to be held in the rooms of the Society at Burlington House, Piccadilly, of flint implements. These implements represent the results of Mr. Reid Moir's activities in East Anglia, and as such will afford a

unique opportunity for archaeologists to realise the debt of gratitude they owe Mr. Moir for the discoveries he has made relating to Early Man in Britain.

Times of opening: Opens Monday, 18th June; hours, 10-5. Saturdays, 10-12. Tuesday, 19th June, 10-1. Tuesday, 26th June, 10-1. Closes Saturday, 7th July, 12 noon.

*Folklore Congress, 1928.*—The Folklore Society, which will celebrate its Fiftieth Anniversary in September next, proposes to signalise the occasion by holding a Folklore Congress which it is intended shall be international in character. An influential Advisory Council has been formed, which includes a number of prominent Continental folklorists. Further particulars of the Congress may be obtained from the Honorary Secretary, Allan Gomme, Esq., 52, Upper Bedford Place, W.C.1.





FIG. 2.—IRRIGATION CHANNEL WITH STEPS AND TUNNEL.

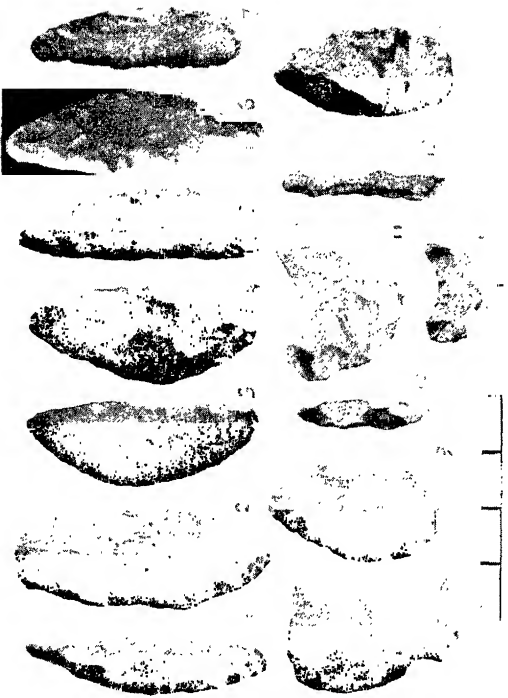


FIG. 4.—IVTH DYNASTY SETTLEMENT FLINTS. SCALE SHOWS INCHES.

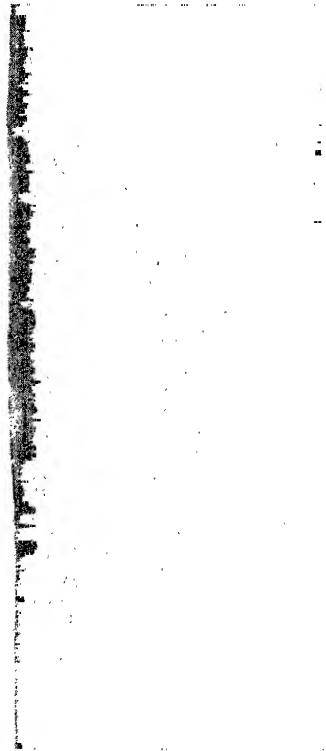


FIG. 1.—TEMPORARY VEGETATION ON BURIED PTOLEMAIC IRRIGATION CHANNEL.



FIG. 3.—AN OLD KINGDOM HUT CIRCLE.

RECENT EXCAVATIONS IN THE FAYUM

## ORIGINAL ARTICLES.

With Plate G.

Egypt: Archæology.

Caton-Thompson.

**Recent Excavations in the Fayum.** By G. Caton-Thompson. With Plate G.

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Pending detailed study and publication of the work done last season in the Fayum, it may be of immediate interest to Fellows of the Royal Anthropological Institute to have a brief record of the conditions governing the work; and a rapid summary of the results.

A second season in the northern desert (1925-6), on behalf of the British School of Archæology in Egypt, had accomplished much of the laborious spade-work of pioneer exploration: the creation of closely-contoured maps of our uncharted desert area gave a firm basis for archæological and geological research, never before attempted in Egypt in such intimate combination. The component parts of the scattered surface flint industry were isolated, and assigned, with attendant geological data, to Mousterian, Neolithic, Predynastic, and Early Dynastic times; to this end many thousands of surface flints were closely levelled, and their relation to shifting lake margins established. Mounds and granaries of the Neolithic population yielded considerable information concerning their material culture, but left unresolved the problem of their date relatively to civilisation, Badarian or later, in the Nile Valley. All I could say with certainty on this point was that direct evidence proved them at any rate earlier than the proto-Dynastic period (*J.R.A.I.*, Vol. LVI, 1926, pp. 321-2). Failing the discovery of graves, which might yield further information on this point, the work was obviously incomplete; and, the patient toil of two seasons' preliminary work, necessitating the survey of about 250 square miles of desert, completed, Miss Gardner and I looked forward to a third season, extending our investigations eastwards, when search for cemeteries was, at last, to be the primary quest.

Return in 1926-7, however, proved impossible; Miss Gardner's professional duties kept her in London, whilst I had arrears of publication; moreover the removal of the British School of Archæology to Palestine rendered it necessary to find fresh sponsors. Our temporary absence lead to disquieting consequences, so seriously affecting, as it turned out, the work ahead that reference to it cannot be altogether omitted.

Tranquil in the tradition which forbids appropriation of another person's work without inquiry as to their intentions to continue it, I found to my dismay, when applying in the spring of 1927 for renewal of concession, that, owing to alleged sensational discoveries (a great prehistoric cemetery; shelter with breccia, ranging from Acheulean to Campigny; rows of dolmens; pile-dwellings, etc.) by Count de Prorok, working unauthorised in our vacated area from the University of Michigan Expedition's base, an American expedition had secretly applied for, and been virtually accorded the N. Fayum concession. Prolonged negotiations with the Dept. of Antiquities, so devoid of prehistorians as to be unable to verify the authenticity of the Fayum discoveries, resulted in acknowledgment of our moral right to continue the work in which we had led the way, but left undefined the area to be assigned to us. The positions of the sites coveted by the Oriental Institute of Chicago were widespread: no attempt was made from that quarter to alleviate our position; and on arrival in Egypt in November we found ourselves re-allotted a restricted concession within the area we had already exhausted both prehistorically and geologically, sandwiched in between Chicago's western concession near Qasr-el-Sagha,

containing the "Palæolithic cave," and their eastern one near Kom Ashim, containing the "prehistoric cemetery" and "dolmens." The black-line square on our map (Fig. 1) shows the area held the first two seasons; the intermittent line halving it the one allotted to us last season. In view of the grave inadequacy of this concession, I applied at once for a second one, covering the very difficult ground at the west-end of the lake: this was granted in January.

In the meanwhile we settled down on our old northern ground to wring such further drops of evidence from it as ironic gods might help us to obtain. They sent torrential rains. The discovery of the Ptolemaic irrigation system resulted, due to growth of vegetation upon the buried, sand-filled channels (Pl. G., Fig. 1). Much as the growth of weeds helped us to trace their course, anything approaching a complete map of the system was possible only by prolonged investigations, making calls upon the detailed knowledge of levels collected in previous seasons. After examination by the Irrigation Department of our preliminary work on the system, we were officially requested by the Minister of Public Works to carry our inquiries as far as possible, and to furnish full plans, sections and reports: our workmen's wages became a Government charge throughout this period, and an additional ten men were supplied us. I was also authorised to carry out any excavations deemed necessary to complete the work irrespective of concession area. The completed work is now with the Irrigation Department in Cairo. Sixteen miles of channels were dealt with (Pl. G, 2), and the region under early Ptolemaic irrigation is shown to be about 21 square miles. This will almost certainly, sooner or later, be brought again under cultivation. Archæologically, the discovery is of considerable importance; the system, dated by coins, appears to be the work of Ptolemy Philadelphus (285-247 B.C.), who is believed to have reduced Lake Moëris to small dimensions. The minimum levels above present lake now disclosed by his irrigation direct a new searchlight upon this problem. We hope to review the question in the light of fresh discoveries (which include secure fixture of the IVth Dynasty level at 140 feet) in a lecture and paper to the Royal Geographical Society in November.

The other unexpected find of the season was that of extensive Old Kingdom gypsum works in the northern hills bounding our concession. A splendid outcrop of pure gypsum in massive formation, nearly a mile long, a quarter broad, and about 15 feet thick, had been extensively used in the IIIrd and possibly early IVth Dynasties (2900 B.C. *circa*), mainly, it would seem, to obtain material for the plaster and mortar required in the construction of the earlier Pyramids and Pyramid cemeteries, less than 30 miles distant by desert route. For this purpose thin plates of translucent gypsum, about  $\frac{3}{4}$  inch thick, which occur in horizontal sheets in the Tertiary clay matrix, were extracted, and reduced to powder for export. Great mounds of gypsum *débris* accumulated, strewn with pottery, and such vast quantities of flint tools that we named the place "Umm es Sawan" = "Mother of Flints." A minor but conspicuous industry was simultaneously carried on in the manufacture of gypsum vases and dishes. These were made from blocks of gypsum obtained from vertical walls of the material, nearly 12 inches thick, running in seams at right angles to the thinner horizontal bands: thus both modes of natural formation were utilised to their greatest advantage.

Over 3,000 defective vases, discarded before completion, were counted in, or on, the workshop mounds, hinting at the great numbers which were exported. They bear the marks of the flint-tools which fashioned them: on the exterior the pecked imprint of pointed hand-picks, found in thousands; whilst the interior was hollowed out by rotatory motion of crescentic grinders (Pl. G., Fig. 4, Nos. 11 and 12), of which about 2,000 were collected in the workshops.

Mr. Lucas's analysis of samples of the gypsum leaves no doubt of the destination of the powdered form. The Pyramid builders' gypsum quarries have never been traced: for, though the material occurs plentifully in the Nile Valley, and was freely used in later dynasties, it is rarely free from a fairly high percentage of sandy or calcareous impurity. Now, a feature of Old Kingdom gypsum-plaster is its remarkable purity. Mr. Lucas finds, for instance, a sample from Khufu's Pyramid to be 97.3 per cent. gypsum, 2 per cent. sand; another, from the shaft-tomb of Hetepheres (mother of Cheops) is no less than 99.5 per cent. pure. Our site yields the 99 per cent. required to solve the question of origin.

That a large population was established in the industry is evident from the size of the *débris* mounds: they were housed in a village of some 250 circular stone shelters (Pl. G, Fig. 3); only the foundations remain, but excavation of about 30 yielded hearths with food remains, sherds, and evidences of gabbro stone-vase making. A good collection of edible plant remains of this early period were collected. Oxen appear to have been the beasts of transport.

My attention has been called to some confusion as to identity of site arising from allusion to a gypsum-vase factory in the Fayum, in a letter to *The Times* of 13th April from Professor Breasted, describing the Oriental Institute's work there. This place, excavated in January by Mr. Brunton and Dr. K. S. Sandford for the Oriental Institute, is none other than the famous "Palaeolithic" shelter! Situated near Qasr-el-Sagha, some 15 miles from the great Umm es Sawan site, an isolated gypsum worker carried on his trade beneath a sheltering cliff, on a little platform about 10 feet square. The early Old Kingdom date of the little place had been known to me since November, when I was shown in Cairo specimens of the "Acheulean" and "Campignian" flints brought from it by Prorok and Bovier-Lapierre, and recognised them as hand-picks and grinders of early dynastic date, and the "breccia" as consolidated gypsum *débris*. As their Palaeolithic authenticity was accepted in Egypt, I sent a private report on the place to the Royal Anthropological Institute. Knowledge of this affair, and of the even more comic "prehistoric cemetery," consisting of entirely natural mounds well known to us, made but slight amends for frustration of our prehistoric research.

In spite of handicaps of ground, however, very considerable advance in knowledge of the Neolithic period has been made. Data have been obtained enabling division into an earlier and a later Neolithic phase. This distinction, already suspected on typological grounds, has now the direct evidence of lacustrine stratigraphy, and lithic distribution. The earlier phase (Fayum "A") represented by mounds and granaries, and connected with a lake 180-190 feet above its present level, is the period of concave-base arrow heads, polished axes, and agricultural flints; the later (Fayum "B"), connected with a lake shrinking to as much as 40 feet below this level, shows a marked deterioration. Old axes are re-fashioned for other purposes, but not replaced; pressure-flaking on implements is frequently confined to edge or tip; a wealth of simple microlithic flints, with double or single edge-batter, become the dominant type. The small tanged arrow-head succeeds the concave-base type, though it too was still sometimes used. An impoverished lake fauna indicates increasing salinity. The culture suggests transformation to a poor fishing population using little pottery, rather than the prosperous agricultural one of earlier times. (The extent of agriculture in period "A" is shown by our discovery of another 115 silos, making a total of 178). Geology emphasises the importance and long duration of the 140 feet stage in the lake's history (see Map, Fig. 1), a point not then pressed in Miss Gardner's paper in *Geological Magazine*, September, 1927: West of Dime it has created a steeply-banked shore-line, about 20 feet high, which can be traced for over 10 miles. Upon the top of this bank we found IVth Dynasty



settlements with sherds, including carinated bowl fragments, flint knives with notched handles (Pl. G, Fig. 4, Nos. 5 and 6) and other typical flints (ditto No. 14). What interpretation, then, can be placed on the fact that this same level is also the later

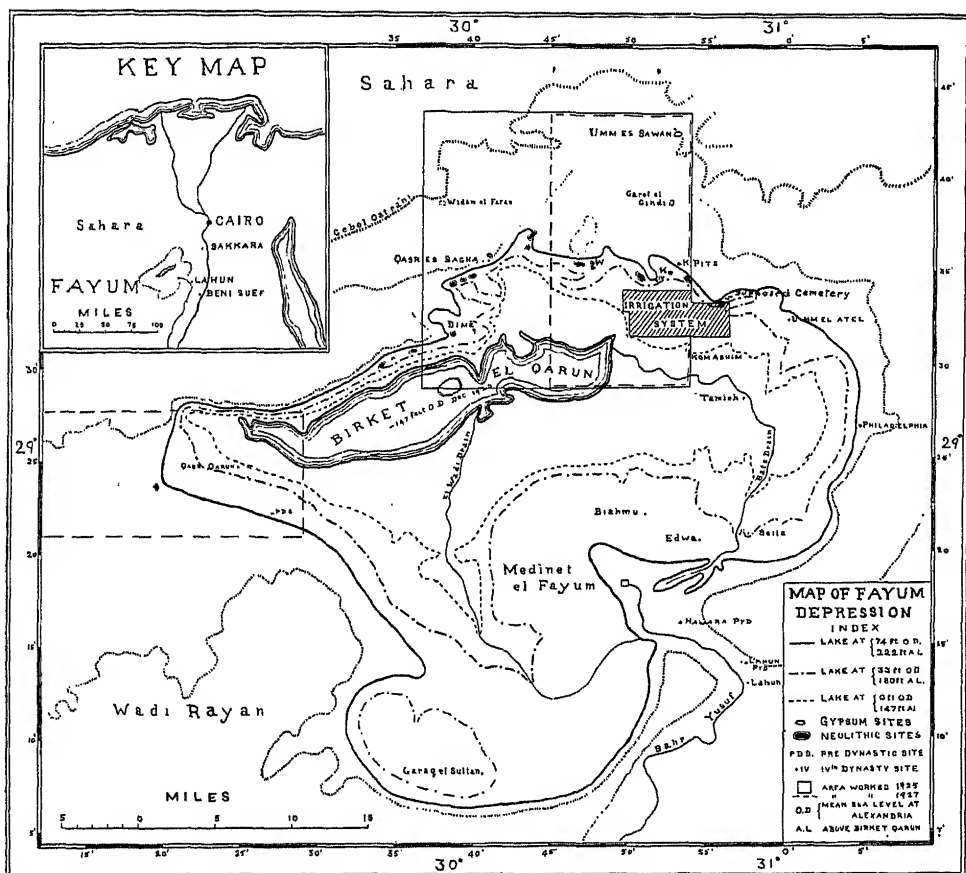


FIG. 1.—THE FAYUM DEPRESSION.

Neolithic one? Most positively must be ruled out the synchronism of Fayum "B" with Old Kingdom times: pottery and flints are absolutely distinct. A big mound of IVth Dynasty settlers near "K" which produced a fine series of flint implements, some of which are shown Pl. G, Fig. 4, and sherds innumerable, yielded but one example of a Neolithic tool: this is a polished flint axe of period "A" which the Old Kingdom people have ruthlessly cut down into a double-notched implement. (Pl. G, Fig. 4, No. 8). Nor have Neolithic mounds or sites, other than surface ones, ever produced a trace of Dynastic objects. Late in the season in our second concession at the west-end (irrigation system and plaster-works kept us north until March) we found a site, small in size, great in inferential value—a settlement of Nile Valley ordinary pre-Dynastic folk of about S.D. 40-50. Its position in relation to surrounding Neolithic culture, and the fact that here, again, the village was devoid of trace of contact with the "A" or "B" populations, has convinced me that attribution of the "Fayum industry" to at least an early pre-Dynastic time is warranted. Doubts on this point, engendered by the granaries, are now quieted by unequivocal discovery of grain in Badarian times: I feel disposed to trust the cultural evidence, and assign the Fayum "A" phase, with its concave-base arrow heads, axes, and pottery with stud decoration (*J.R.A.I.*, Vol. LVI, p. 312) to a period, possibly prior to, but

relatively close to the Badarian; whilst the degradation of Fayum "B," connected, it would seem, with changing physiographical conditions, may possibly extend the period of this local Neolithic on into early pre-Dynastic times.

Miss Gardner's geological results bear out her former conclusions. The discovery of shells, new to Egyptian palæontology, connected with the 222 feet lake level, promises to reveal interesting affinities. The expedition consisted of Miss Gardner (geologist and surveyor); her brother, Capt. Guy Gardner (mechanic and draughtsman), who gave valuable archæological assistance in the Ptolemaic work; and myself. With us were six native workmen, inherited from the British School of Archæology, and four others lent by the Egyptian Exploration Society. Work ceased on 27th April, after 10 days of intense heat, with tents up to 122° Fahr.

Deep thanks are due to the President and Council of the Royal Anthropological Institute for standing sponsor to the expedition; to the Hon. Sec. and Fellows of the Institute; to Newnham College; the Percy Sladen Trustees; the Royal Geographical Society; the Geological Society; the Egyptian Ministry of Public Works; Sir Flinders and Lady Petrie, and to many friends, known and unknown, for their financial support, without which nothing could have been accomplished.

G. CATON-THOMPSON.

## Melanesia: Sociology.

Baker.

**Notes on New Hebridean Customs, with Special Reference to the Intersex Pig.** By John R. Baker, M.A., D.Phil.

81

During my visits to the New Hebrides (including the Banks Islands) in 1922-3 and 1927, under the Percy Sladen Trust, I made a biological study of the intersexual pigs which abound in certain of the northern islands. Nowhere in the rest of the world are intersexual mammals so abundant. The results of my investigations will appear shortly in the *British Journal of Experimental Biology*.

In the external genitalia of these monstrosities there are all intermediate conditions between what is almost exactly the female anatomy and a fairly near approach to the male. As regards the internal genitalia, only male organs occur (testes, epididymes, vasa deferentia, vesiculæ seminales, etc.); these are sometimes fairly well developed, sometimes rudimentary. Sperms are rarely present, and the intersex cannot reproduce. There are no female internal organs. There are tusks like those of the male. The natives knock out the upper canines in both males and intersexes, thus allowing the lower canines to grow indefinitely owing to their not being worn against the upper ones. In perhaps seven or eight years a complete circle is formed by the growth of the lower canines, the point of the tusk often entering the bone of the lower jaw near its own root.

While engaged on these biological investigations, I made enquiries of the natives about the social significance of the intersex, and I record my findings in this paper.

It is remarkable that anthropologists have not paid more attention to the intersex pig. Codrington mentions it in his book, "The Melanesians."\* A man dreams of "a stone in shape like the distinguishing part of a valued kind of pig." In a footnote to this, Codrington says: "In certain breeds of pigs in the Banks' [sic] Islands and New Hebrides, which are much valued on this account, there occur individual females which simulate the male sex. These are in the Banks' Islands *rawe*; they furnish the finest tusks." On another page he says: "Before admission can be obtained to the Great Tamate [a secret society], the candidate or his friends has to *usur* [qualify by payment] with a pig of the valued kind called *rawe*."

\* Codrington, R. H. "The Melanesians." Oxford: Clarendon Press, 1891. Pp. 57 and 80.

Rivers, in "The History of Melanesian Society,"\* mentions the striking of the head of an intersex pig at initiation into one of the divisions of the Sukwe Society in the Banks Islands. He also mentions the payment of an intersex pig at initiation into the *Tamate liwoa* society and its use in the purchase of a bride in the same group of islands.

I shall first describe my findings in Sakau, the N.E. peninsula of the island of Espiritu Santo.

A male pig is called *nëvor*,† a female *ndrasa*, and an intersex *ndrē* or *nërē*. The *ndre* is worth about half as much again as a male. Females are of very little value.

The chief use of the *ndre* lies in its function in the pig-killing feasts by which a man rises in rank. There are five main ranks in Sakau society; these are, in ascending order: *nëvok*, *vonërē*, *vüriar*, *vüriaru* (or *levüs*), and *vüstēr*. Many men never attain even the lowest. In order to attain the lowest rank, or to rise to a higher one, a man must kill a certain number of male pigs and a certain number of *ndre*. Usually a man has not enough pigs of his own, and has to borrow from others. If he is unpopular, other men will not lend him pigs, and thus as a rule only fairly popular men can rise in rank. It is a remarkable fact that no man ever sets out to breed pigs on a large scale, making special gardens in which to grow food for them. I pointed out to the natives that this is what a white man would do, if he wished to rise in rank in their society. This was received with laughter as an obvious absurdity. It is remarkable that their minds are so different from ours, that this suggestion seemed to them absurd; yet I had precisely the same experience when I made the same suggestion on another island.

Most female pigs are killed directly they are born. A man seems to get his wealth in pigs from four main sources:—

1. His father, if wealthy in pigs, may give him some.
2. When his daughters marry, the bridegroom pays him in pigs.
3. He extorts pigs from others by pretending to possess magical powers.
4. Occasionally he allows a female to come to maturity and breed.

The third method is very important. A man of strong character is much feared by the other natives, who imagine that if he can obtain any of the remnants of their food he can weave a spell over them and thus injure their health or even cause them to die. Père Ardouin told me that death may actually be caused in this way by suggestion. The victim is told that if he pays a pig, he will not be visited with ill-health or death; and he readily parts with one. A man called Rewar, at Port Olry, has a very great power of this sort over the people of his village, and is rich in pigs as a result. Even the Christians in the village are scrupulously careful not to leave banana skins, etc., lying about, lest Rewar should find them. All such dangerous relics are hidden, thrown into the sea, or fed to pigs.

It thus usually comes about that only those men who are feared by the others and are fairly popular rise high in rank.

With regard to the number of males and *ndre* that must be killed at each change in rank, accounts from different natives are somewhat contradictory; but it appears that to become a *nëvok* it is necessary to kill a hundred small *ndre* and two adult ones; a *nëvok* must kill many adult males to become a *vonërē*; a *vonërē* must kill many adult males and two adult *ndre* to become a *vüriar*, and so must a

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\* Rivers, W. H. R. "The History of Melanesian Society." Cambridge University Press, 1914. Vol. I, pp. 38, 80, 98, and 107.

† In spelling native words in this paper, I use the Italian vowel sounds. In addition, I use "ë" to represent the indefinite sound of the "e" in the French "le."

*vüriar* to become a *vüriaru*; and finally a *vüriaru* must kill ten to twenty adult *ndre* to attain the highest rank of all, *vüster*.\* At each change of rank a new men's house (*nëviul*) must be built. Only males may enter the *nëviul*. Boys are not excluded, but women may never enter. The *nëviul* is not subdivided according to rank.

At each change of rank except the first a night-long dance takes place, at which there is said to be considerable sexual licence. In company with my wife and Mr. J. de H. Morel, I have witnessed one of these dances at Yekul, on the occasion of the rise in rank of Tëkara, the local *vonëre*, to *vüriar*.

A three hours' walk from the mission village of Hog Harbour brought us to Yekul soon after sundown. The latter part of our journey was along a specially prepared path, differing from the ordinary track in being from two to three yards wide instead of only just wide enough for one person. As we approached, we heard wild shrieks and two oft-repeated notes on pan-pipes. In the darkness we could just perceive a native hiding in the forest beside the path, evidently a sentry to warn the dancers of the approach of any unwanted stranger. He stood stock still until we had passed, and then we heard him crashing through to get to Yekul before us.

On arrival we kept very much in the background, lest the presence of white people should be resented. The path led us up to one side of an oval enclosure perhaps 70 yards long, in which the ceremony was taking place (see Fig. 1). We sat down just within the fence of closely-placed 3-foot posts which bordered the dancing ground, and watched the spectacle by the fitful light of a great bonfire which burnt in the middle. The ground within the fence was absolutely bare, having been cleared of every little weed for the purpose of the ceremony. A house†

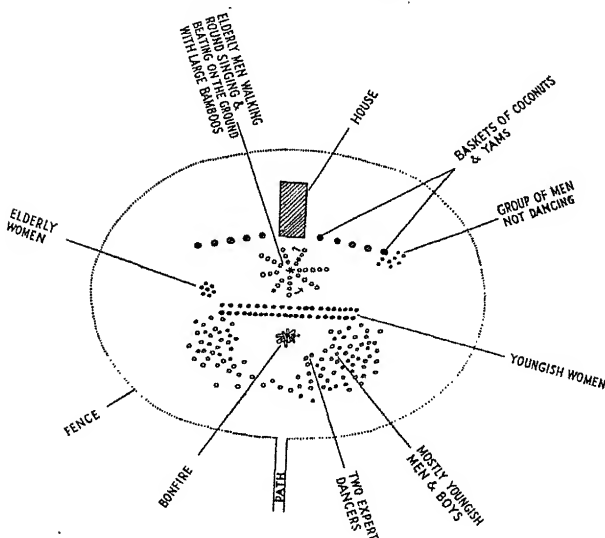


FIG. 1.

filled with yams and coconuts for the dancers. Each basket had been made by fixing some half-dozen 20-feet saplings upright in the ground so as to form a cylindrical framework about 2 feet in diameter, and binding these round to a height of 8 or 9 feet with rope-like lianes. Each sapling was stripped of branches and leaves except just at the top, where a little bunch was left, giving the whole structure a most grotesque appearance.

No one could have witnessed what was going on in the arena which I have described without a thrill of

intense excitement passing down his spine. A couple of hundred savages, each one with white fowl-feathers in his hair, his face hideous with bright pigments, a great club or a musket in his hand, and a twisted garter just below each knee, were throwing themselves into the dance with such abandon as only savages can command.

The older men had arranged themselves in lines radiating from a common centre, about which they revolved, chanting what is said to be their history and

\* Sometimes a man becomes *vüriatel* after *vüriaru*.

† Presumably a *nëviul*.

beating time upon the ground with 8-foot bamboos. They have several melodies, which all seem to have something in common. The following, though not an exact reproduction of any, gives an idea of the music :—



The young men surrounded the bonfire, facing it, and danced about, raising their knees high in the air, and then stamping loudly upon the ground. Occasionally they uttered loud shouts in syncopated time with the singing of the older men, but they did not sing themselves. All the time they were gradually approaching the bonfire. When they had nearly reached it, they would suddenly rush back, screaming as though afraid. Their gradual approach would then begin again.

The women were in two long rows, with linked arms, and swayed up and down all night long, singing with shrill voices.

Shortly after the beginning of the ceremony, Tekara strode proudly round and round the arena outside the dancers, with all his near relatives—male and female, adult and young—following in his wake. Last of all was his mother, holding a coconut before her to represent the great Tekara as a baby.

Before our arrival Tekara had killed ten pigs, of which two had tusks in the form of complete circles.

*Ndre* are not eaten at these ceremonies; indeed, they are never eaten except by women and children (and possibly by men who have never become *nēvok*). Men also never eat female pigs. Since the *ndre* cannot reproduce and is scarcely ever killed except at a pig-killing ceremony, it is evident that it has no real value at all; the fact that it is worth more than a male is due to its being less abundant yet required in these ceremonies.

The leaves of crotons are required during part of the ceremony to cover the bodies of the pigs that have been killed. At Lowerie I saw the croton-garden belonging to Yaro, the *viriar* (?) there. It was about 30 yards in diameter and beautifully weeded and fenced. Yaro's house is the largest house I have seen in the New Hebrides, being about 90 feet long by 18 wide by 8 or 9 high. Within it I counted no fewer than 101 lower jaws of well-grown male pigs, which had been killed in his rank-changes. He told me that he had thrown away those of the *ndre* that he had killed.

As many as twenty pigs are paid for a wife. In one case of which I heard, the largest pig of the twenty paid for a wife was a large-tusked *ndre* worth £24 in our currency.

Pigs are the standard currency of the people of Sakau. As was mentioned before, they lend them to one another; and thus they become indebted to one another in a most complicated way. If a small pig is lent, a pig of the size which it would have attained in the intervening period must be returned later on. The value of a male or *ndre* lies almost entirely in the tusks. The more nearly they approach a complete circle, the more valuable is the pig, even if the pig itself is a small one. If one of the tusks is broken, the pig is almost worthless. After the death of the pig, however, the tusks have little value.

It is not too much to say that pigs, especially intersex pigs, are the chief interest of the people of Sakau. Mr. T. O. Thomas has seen a woman suckling one at her breast. They have a remarkable classification of the *ndre*, based upon the anatomy of the external genitalia. The names are mostly derived from the resemblance of the external genitalia to parts of animals and plants. Starting with

the most female and ending with the male, the names are *ndre rasa* (feminine intersex), *ndre rineth* (sprouting coconut intersex), *ndre narsögh* (sewn-up intersex), *ndre ghara* (fruit-bat intersex), *ndre nau* (rat intersex), *ndre ghor-ghor* (hidden intersex), and *ndre selet* (worm intersex). The anatomy of each is described in my paper in the *British Journal of Experimental Biology*.

I will now add notes about the sociology of the pig in other parts of Espiritu Santo and in other islands of the New Hebrides.

*S.E. Espiritu Santo*.—The great significance of the pig to the natives of S.E. Espiritu Santo is shown by the following incident:—

There had been some fighting among the people, and a young chief had installed himself on Mt. Turi and was terrorising the people of the surrounding villages. Mr. Anderson, the Presbyterian Missionary of eastern Espiritu Santo, decided to go to the Mt. Turi to try to put an end to the trouble, and Mr. T. T. Barnard and I accompanied him. We went inland and spent some days in visiting the oppressed villages. From each we took one or two of the chief men and sent a message to the oppressor that we should bring them all to a conference at his village on the mountain. There was no disorder when we arrived with our long file of the oppressed in the enemy's village. The chiefs all sat down on their haunches and the conference began. One after another spoke, and no settlement seemed possible. By means of an interpreter Mr. Anderson did all he could to settle the outstanding difficulties, but to no avail. The discussion now went on among the people themselves. I was thinking that the matter would never be amicably agreed, when suddenly we were told that a satisfactory settlement had been reached. The oppressor was to give a pig to the most influential of the oppressed chiefs, and the latter was to give a pig to the oppressor! This seemed perfectly conclusive of the trouble to everyone, and his late enemies as well as ourselves slept in the oppressor's house. Such is the importance attached to the reciprocal gift of a pig!

The intersex is called "*ra*" or "*ravē*" in S.E. Espiritu Santo, and there is a classification differing from that used in Sakau. The ranks, and pigs killed to attain each, are given below:—

- |               |   |                |   |   |                                     |
|---------------|---|----------------|---|---|-------------------------------------|
| 1. Nevak      | - | -              | - | - | No intersexes killed.               |
| 2. { Naforles | } | (alternatives) | } | } | Only intersexes killed.             |
| Naforbaberik  |   |                |   |   |                                     |
| 3. Karaora    | - | -              | - | - | One intersex and many males killed. |
| 4. Kara       | - | -              | - | - |                                     |
| 5. Daridari   | - | -              | - | - |                                     |
| 6. Ase        | - | -              | - | - | One intersex and 100 males killed.  |
| 7. Levus      | - | -              | - | - |                                     |
| 8. Mol        | - | -              | - | - |                                     |

*Mafia Island* (a small islet off the east coast of Espiritu Santo). The people of Mafia specialise in trading in pigs. One day they brought four large intersex pigs with complete or nearly complete tusks to Hog Harbour. In exchange the people of Sakau gave them 100 very small intersexes and 20 large ones with small tusks. (I myself counted these pigs.) The Mafia men took these 120 intersexes in their boat to the islet of Tutuba (to the S.E. of Espiritu Santo), where they were required for a rank-change ceremony, receiving about eight complete-tusked intersexes in exchange. They thus made a profit of four complete-tusked intersexes.

*Mota* (Banks Is.).—The intersex pig is called *ravē*.

*Gara* (Banks Is.).—The intersex pig is called *rau* or *rolas*. In this island all the intersex pigs have external genitalia approximating more or less nearly to the female type, so there is no classification corresponding to that of Sakau. Both males and intersexes are classified as *mung-lin*, *vēwut*, or *mbēte*, according to the size of

the tusks. A sow that produces intersexes is called *wurupkwaval*, and one that does not is called *wurupowor*. All the daughters of *wurupkwaval* are said to be *wurupkwaval* also. A *wurupkwaval* is worth twice as much as a *wurupowor*, a boar with good tusks twelve to twenty times as much, and a *rau* with good tusks thirty times as much.

*Merelava and Merig* (Banks Is.).—In these islands there were formerly no intersexes, so intersex-producing sows were imported.

*Omba* (= Aoba).—Intersexes are said to occur.

*Ambrim*.—Intersexes are called *teret*. They are not used in pig-killing ceremonies and have no special value. Men as well as women eat males, intersexes and females, but at grade-changes only males are killed. Intersexes are not abundant, and since their upper canines are not knocked out, the lower ones do not grow to form circles. (I have not been in Ambrim, and since I obtained all this information from one native of the island it cannot be regarded as certain.)

*Tongoa*.—Intersexes are called *pulpul*. They were formerly killed at grade-changes, but the island is now Christianised and the ceremonies no longer occur. They are not plentiful.

In Malekula, Efate and the islands south of Efate, intersexes do not occur.

In conclusion, I wish to thank Mr. and Mrs. W. Anderson, the Rev. R. E. Tempest and Mr. T. O. Thomas for much help while in the New Hebrides.

JOHN R. BAKER.

## Ireland: Archæology.

Moir: Burchell.

**The Sligo Artefacts.** By J. Reid Moir and J. P. T. Burchell.

82

During a recent visit to Sligo, one of us, Mr. J. P. T. Burchell, accompanied by Mr. Blake Whelan, met Professor Macalister, Dr. Praeger, Mr. Hallissy and Dr. Adolf Mahr at Rosses Point village. As is known, the two former investigators have claimed that the specimens of limestone implements collected and figured by us ("The Early Mousterian Implements of Sligo, Ireland." Harrison: Ipswich, Suffolk) owe their forms and flaking to wave-action and may be found on any limestone beach. Professor Macalister, therefore, was invited to visit the western shore of Coney Island where a storm-beach, over half a mile in extent, is present. A search of this area, however, having failed to reveal any specimens such as are in dispute, and in view of Professor Macalister's assertion that the limestone there exposed was of extreme hardness and not susceptible to natural flaking, a visit was paid to the northern side of the island. Here, opposite the cave mentioned in our Memoir, Professor Macalister picked up two pieces of limestone rock which he claimed as being similar to those figured and described by us, and upon being asked whether these two specimens might be retained and taken back to London as representing the Royal Irish Academy's contentions, he readily agreed. We have now subjected the two specimens picked up by Professor Macalister to a close examination, and have compared them with those collected by us, and we wish to state with all the emphasis at our command, that, except for the fact that the members of both series are composed of limestone, they bear no resemblance to each other. In the one case, the specimens have been quite obviously fractured by skilful blows giving rise to large clean flake-scars, while in those picked up by Professor Macalister, the fractures are uneven, clearly natural, and have been formed, apparently, by some form of thermal disruption. It is, in fact, by no means easy to imagine any two series of objects of the kind which are more dissimilar, and we regret that Professor Macalister should believe that we are so lacking in a knowledge of these matters as, under any circumstances, to regard chunks of rock such as he picked up, as having been flaked by man. We believe also, that if Professor Macalister had examined, which he has not yet done, the specimens

collected by us, he would never have allowed himself to be guilty of such a gross error of judgment. As this is a serious matter and one which affects the very foundations of prehistoric archæology, we deposited the two specimens picked up by Professor Macalister in the Department of British and Mediæval Antiquities at the British Museum immediately following their exhibition before the Royal Society at Burlington House on Thursday, 17th May, in order that all those interested may be able to go there and decide for themselves whether the similarity claimed by Professor Macalister has, or has not, any basis in fact.

J. REID MOIR,  
J. P. T. BURCHELL.

### Ireland: Archæology.

Breuil.

**Les Calcaires taillés de Sligo, Irlande.** *Par M. l'Abbé de Breuil.*

83

Ayant eu l'occasion d'examiner une importante série des calcaires "taillés" de Sligo, Irlande, conservés par MM. Burchell et Reid Moir, et au British Museum et à celui d'Ipswich, je pense être en mesure d'exprimer une opinion sur la question discutée de leur "travail" ou de leur origine naturelle. Cela m'est facilité par le fait que, tant de la grotte de l'Observatoire de Monaco, que de tous les niveaux paléolithiques de celle du Castillo (Santander) j'ai eu l'occasion de voir et de manier de nombreuses séries trouvées *in situ* dans des niveaux bien définis. Voici quelles sont mes conclusions :

1. Les deux objets de "thermal" origine, déposés au British Museum par les opposants à l'origine humaine de ces pierres, ne sont pas travaillés, ni brisés par des coups mécaniques quelconque. Ces deux pièces n'ont aucun rapport avec l'ensemble des autres objets qui m'ont été présentés, et ne sauraient fournir aucun argument à leur sujet.

2. Tous les autres objets sont fracturés par une force mécanique très puissante, naturelle ou humaine, procédant par percussion très violente en un point précis et limité; la fracture en question s'est produite en formant un bulbe souvent étoilé d'un grand nombre de stries rayonnantes, qui est normal à toutes fractures par percussion sur un calcaire compacte.

3. Un petit nombre de ces éclats, enlevés d'un seul coup, vertical à un banc calcaire, pourraient, à la rigueur, être expliqué par la chute d'un grand bloc d'un niveau supérieur sur le rebord saillant d'un lit inférieur.

Mais un grand nombre d'éclats, non seulement ont un plan de frappe et un plan bulbé d'éclatement parfaitement défini, mais montrent sur le côté dorsal les vestiges négatifs de plusieurs enlèvements plus ou moins analogues, procédant de coups aussi puissants et bien dirigés donnés de droit, de gauche et d'en face. Plusieurs même présentent à ce point de vue un aspect aussi compliqué qu'un éclat de Northfleet enlevé après préparations d'un "tortoise-core." Cela ne saurait à mon sens être aucunement produit par des causes naturelles, pression ou action des vagues. On peut constater, sur un petit nombre de pièces, des retouches secondaires.

4. Il existe encore de grands fragments aplatis, dont le bord a été débité par des percussions données, non verticalement au bord, mais presque dans le plan de largeur de ces blocs, qui me paraissent de gros nucléi.

5. Je ne pense pas que la technique de débitage de ces blocs de calcaire très dur soit Moustérienne, car je n'ai pas remarqué de vrais préparations de plan de frappe.

6. Il appartient à la géologie d'en fixer l'âge; mais à priori, je ne vois aucune impossibilité à ce qu'ils appartiennent à une période très reculée du paléolithique.

H. BREUIL.



Ireland: Archæology.

Charlesworth.

**Ireland in Pleistocene Times.** By Professor J. Kaye Charlesworth.

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Under this caption in MAN, 1928, 54, Mr. C. Blake Whelan dissents from certain of my views expressed in a small article in the *Irish Naturalists' Journal*, written for the guidance of non-archæological readers. His note, short as it was, contained a surprising number of erroneous assertions and assumptions, to some of which I wish briefly to refer. First, may I dissociate myself from his correlation—outer Drift=Mindel, newer Drift=Riss—to which those who have not seen my innocent paper may think I subscribe. I doubt very much the advisability or usefulness, in our present state of knowledge, of attempting correlations of British drifts with the Alpine glacial succession of Penck and Brückner, uncertain as this in some important respects has become in recent years. In these matters it has always seemed to me more profitable to think in terms of North-West Europe. Were I, however, to link up our British drifts with those of the Alps, the particular correlation accepted by Mr. Whelan would be the last one I should adopt.

May I also repudiate the belief, attributed to me, in tundra conditions during true interglacial times? Such an affirmation in these days would be foolish indeed.

Mr. Whelan's contempt for our old-fashioned belief in the stubborn fact of the coincidence of the limit of glaciation in England with the Thames-Bristol Channel line I leave to fellow geologists to appreciate. It recalls the argument in the new book on "The Early Mousterian Implements of Sligo," by Messrs. Burchell and Reid Moir, that Palæolithic Man is to be expected in Sligo on the ground that this region lies within one degree of the latitude of Cromer (I quote from memory) and on the longitude of Spain! When such ingenious arguments are presented for our acceptance, it is little wonder that simple, unequivocal geological evidence, as that bearing upon the Sligo sites, should fail to carry its due weight or find appreciative understanding.

Concerning the Sligo "implements" and Mr. Whelan's dictum that "the solution of the rock-shelter problem will entail a longer view and more subtle arguments than a theory of recent sea-caves and angular beach material," it suffices to say that the complete answer to that statement is given in the recent letter to *Nature*\* by Professors O. T. Jones and P. G. H. Boswell, who, having kindly acted as impartial assessors of the geological evidence and carefully investigated the problems on the spot, agree with the interpretation of the geology of the sites arrived at by Professor Macalister, Dr. Praeger, Mr. Stelfox and myself. Long views, based upon completely erroneous comparisons between the drifts of East Anglia and Sligo—the one area situated 200 to 700 miles from its centres of ice radiation, the other almost certainly within 25 to 30 miles of the main iceshed—and founded upon such subtle arguments as the astronomical positions of Sligo, East Anglia and Spain and their relation to the glaciations have led to the present archæological *impasse*. Any human artefacts discovered between or in the drifts of East Anglia demand, of necessity, an ice-oscillation only; in Sligo, virtually a complete deglaciation. The glacial analogue of East Anglia is manifestly to be sought in Ireland, not in Sligo, but in the extreme south.

Mr. Whelan seeks to justify his faith in the existence of Palæolithic Man in Sligo on the following grounds:—

- (1) The greater altitude of the land during glacial times.
- (2) The probability of ancient caves in the area.
- (3) The evidence of the Keshcorran caves.
- (4) The reality of interglacial periods in Britain.

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\* June 2nd, 1928,

(5) The evidence of Upper Palæolithic Man at Inchnadamph in N.W. Scotland. To each of these, in order, the following critical remarks may be appended:—

(1) Mr. Whelan speaks *simpliciter* of the land "extending as it then did to the [outer edge of the] continental shelf." Of all glacial uncertainties, this surely is the most uncertain. Some glacialists believe in deep submergence; some in a much greater extent and altitude of the land than the present; while others, again, disbelieving in constancy of level, demand vertical oscillations of considerable magnitude. Here assuredly is need for caution. The state of uncertainty of our knowledge and the urgent need to find a satisfactory solution are clearly revealed in the choice of this very question as the principal theme for discussion at the forthcoming International Geographical Congress.

(2) The possibility of the existence of such caves is not denied. What is denied, and that on the clearest geological grounds, is that these particular shelters are of the great antiquity which their postulated early Mousterian age would require.

(3) In the Keshcorran caves there have been found only glacial forms, as reindeer and lemmings. Where is the interglacial fauna? At present we have not even a hint of its existence.

(4) Where are the interglacial deposits in Scotland and N. England with their warm fauna and flora? Where are they in Ireland? Kirmington, the only probable occurrence, lies on the border of the Newer Drift, and incidentally, be it noted, implies a higher level of the sea than to-day. Glacial geologists can scarcely be expected to build their superstructure of glacial and interglacial horizons upon such insecure foundations as the Sligo "implements."

(5) When Mr. Whelan speaks of the Inchnadamph discoveries, he touches upon a matter of wider import. From the conclusions arrived at by the Scottish investigators, I wish respectfully to demur, as, in my opinion, they are based upon very faulty deductions. Upon what evidence does this determination of an Upper Palæolithic Age rest? Not upon artefacts, but upon such collateral evidence as the presence of an Arctic fauna (reindeer, Arctic fox and cave bear), the state of fossilisation of the bones and the existence of a neighbouring glacier. The state of fossilisation of the bones is extremely inconclusive. It could scarcely warrant a distinction between an Azilian and a somewhat earlier Upper Palæolithic Age. The other material merely proves a cold climate in Scotland at this date and the survival of these Arctic forms to such a late period. On such grounds an Upper Palæolithic Age could be assigned to the Arctic regions of to-day.

We know that glaciers came down to sea level during the period of the 50-foot Azilian beach in West Scotland. It is surely reasonable to assume that a cold mammalian fauna lived in the adjacent regions. In other words, there is nothing in the Inchnadamph discovery which is inconsistent with an Azilian Age.

The question of Sligo Man is of vital importance to British Archæology and Glaciology, for, should the Palæolithic age of these "implements" be indisputably demonstrated, we should have proof, not only of the occurrence of Palæolithic Man in Sligo, which is of local interest only, but of a complete deglaciation of Ireland, probably of Britain, during glacial times, and not merely of an oscillation of the ice-margin such as would be reconcilable with Palæolithic finds in East Anglia. Should, however, as seems to me quite certain, the *provenance* of the Sligo "implements" prove beyond all doubt their non-human origin, then the Sligo controversy will have furnished still another example of the extreme need for caution and circumspection in drawing conclusions from typology alone, especially in new material.

In the light of the incontrovertible geological evidence, I should like to conclude this note by appealing to those archæologists who have expressed an

opinion upon the authenticity of these "implements" to examine the question anew on the actual sites themselves, which I submit is the only place where a final decision can be reached.

J. KAYE CHARLESWORTH.

### India: Archæology.

Seton-Karr.

#### **Note on Prehistoric Implements in some Indian Museums.** By

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H. W. Seton-Karr.

Palæolithic axes and other heavy implements hitherto found in India have been mainly of quartzite and rather roughly fashioned. A fine series comprising the main types can now be seen at the new Prince of Wales's Museum at Bombay. Those I found in the valley of the Penaaar river and its tributaries have now been added to them, including only selected specimens, and the whole may now be said to form the best collection of man's earliest work in India in any of the world's museums, as is only proper and suitable. I have not, however, seen the Calcutta Museum lately.

I recently re-arranged, at the request of the Director, the very large series in the Madras Museum. In this museum all, except a series of the best selected specimens, including neolithic polished axes, and the heavy type of hammers with finger-holes from the Ken river, have been put away for reference by students when required. They were previously unlabelled and only partially catalogued. They now fill two large cases, and are exclusively from South India. They had previously occupied twenty cases.

I was recently the guest of the Maharajah of Jhallawar in Rajputana, and found about a dozen fairly good palæos. of St. Acheul type in some nullahs on rising ground. They have not, I believe, been previously found in this part of India. They are now in the small State Museum at Jhulrapatan. I enclose a photograph of seven. The material of which they are made is quartzite. Another fine representative collection of Indian prehistoric implements was in a room at the Victoria Museum in Bombay, whence they had to be removed owing to a defect in the roof. These are now in two cases in another part of the building. In the fine Museum at Colombo, at the Director's request, I arranged and labelled the Ceylon implements. The majority are pigmy microliths of quartz crystal. There are also large scrapers and side choppers of quartzite.



PALÆOLITHIC IMPLEMENTS FOUND IN RAJPUTANA FOR THE FIRST TIME.

Most of the palæolithic portion of these collections, including a small series I presented to the Gaikwar's Museum at Baroda, were lying in gullies and nullahs, and had apparently been washed from their original positions during the course of many thousands of years, as some were much water-worn or patinated with incrustations. A few had seemingly been thrown up from well excavations in boulder-bearing deposits, all of the so-called red lateritic character.

H. W. SETON-KARR.

## REVIEWS.

Britain: Anthropology, Physical. Parsons. *Anglo-Saxon Skull Contours*. By F. G. Parsons, F.R.C.S., F.S.A. 86 Occasional Papers of the Royal Anthropological Institute, No. 9. Published by the Institute at 52, Upper Bedford Place, London, W.C. 1. Square quarto (11 in. x 9 in.). Price 21s. (To Fellows of the Institute, 12s. 6d.)

This addition to the Institute's series of Occasional Papers differs both in scope and in make-up from its predecessors. It consists of a portfolio containing sixty-six half-scale representations of Anglo-Saxon skulls, in front, side, and top view, with only the necessary letterpress to identify each specimen.

The representations are outline projections made with the dioptograph, which have obvious advantages over photographic reproductions, and make detailed comparisons easy. As each specimen is recorded on a separate sheet of stout, carefully selected paper, the series is a valuable addition to the equipment of the anthropometric laboratory, and should be in wide demand for teaching purposes. Nothing can wholly replace direct study of the skulls themselves; but the practical difficulties of the transport and handling of objects so fragile are notorious, and for most purposes this series of projections will be found sufficiently accurate and detailed.

In a short introduction, Professor Parsons gives sufficient information as to the precautions with which even so carefully executed a series must be used, to eliminate risk of error when the projections are compared with museum specimens or casts. He also indicates that this series is only a first instalment of his materials; and the portfolio cover is ingeniously devised so as to contain the further series which it may confidently be hoped that the reception of the present collection will encourage the Royal Anthropological Institute to undertake.

J. L. M.

Ægean: Archæology. Casson. *Essays in Ægean Archæology*, 87 presented to Sir Arthur Evans in honour of his 75th birthday. Edited by S. Casson. Pp. 142. Frontispiece, 25 plates and 12 figures. Oxford: at the Clarendon Press. 1927. Price 15s. 0d.

The discoveries of Sir Arthur Evans at Knossos have been so remarkable, and of such importance in restoring the early chapters of history, not only in the Near East, but throughout Europe, that it is surprising that his fellow-workers and pupils have waited until his seventy-fifth

birthday before presenting him with a "birthday-book." The volume that has now appeared is made up of contributions from thirteen scholars, of whom one is a German and one a Greek. The writers are well-known students of Ægean and Egyptian archæology, and the essays will be welcomed by all interested in the early history of the Near East.

H. P.

Egypt: Archæology. Scharff. *Grundzüge der Aegyptischen Vorgeschichte*. By Alexander Scharff. 88 *Morgenland*. Heft 12. Pp. 69, pls. 16 and 1 map.

This is not only a clear summary of the whole prehistoric period of Egypt, but also an important contribution towards the elucidation of some of its many problems. Dr. Scharff brings forward many potent arguments to prove that the so-called "First prehistoric period" is entirely African in origin and belongs only to Upper Egypt. These arguments are founded chiefly on the pottery and its distribution, and, to a lesser extent, on the stone vases and flint implements. He accepts and uses Petrie's system of Sequence-dating as the simplest and most accurate method of working out these problems. He places the "Second prehistoric period" to the north, and equates it with the introduction of sun-worship. The connections of this civilisation were with the north; the characteristic wavy-handled vases are closely allied with the Palestinian forms; lazuli and obsidian are products introduced from the north; and the materials required for glazed ware are found in the Delta. In the late prehistoric and early historic periods, Babylonian influence is visible, and the earliest slate palettes show Cretan elements mingled with the indigenous African; in other words, the connections are again with the north. Dr. Scharff is strongly of opinion that the north overran the south before the beginning of history, and notes as one of his proofs that the names of the points of the compass show that the invaders were proceeding southwards. He also discusses the early chronology, and claims that the Egyptian calendar with the five intercalary days was introduced in the IIIrd dynasty under Zoser. He ends this extraordinarily interesting paper with a short account of the plastic art, showing that it belongs almost exclusively to the First Prehistoric period, and that the art of the whole historic period was derived from the early forms.

M. A. MURRAY.

Egypt: Archæology.

Spiegelberg.

*The Credibility of Herodotus's Account of Egypt.* By William Spiegelberg. (Translated by A. M. Blackman.) Oxford: Basil Blackwood. Pp. 40. Price 2s. 6d.

This pamphlet is one of a series of monographs called *Orient und Antike*, and was originally delivered as a lecture. Professor Spiegelberg points out that Herodotus merely includes Egypt as part of the Persian dominion; that he stayed about three months and a half in the country, i.e., August to November; that as he had no introductions he could come in contact with very few Egyptians, especially those of the higher and more educated classes; and that as he could not speak the language he was dependent for his information, as is the modern tourist, on the intelligence of the dragoman or of any of his fellow-countrymen whom he happened to meet. Professor Spiegelberg also points out that, in these circumstances, Herodotus was likely to meet only the humbler and more ignorant priests, probably those whose duty it was to take tourists like himself round the temples; the learned theologians and "the great" princes of the Church, in their mandarin-like pride, would not have condescended "to talk to an inquisitive Greek." Professor Spiegelberg agrees with most Egyptologists that the descriptions which Herodotus gives of what he saw with his own eyes, may be relied on; while the tales told to him by the "priests" and other informants, though often utterly inaccurate, are of great value, for "he has shown us how the memory of Egypt's great past still lived on among the people . . . Without Herodotus we should never have been able to construct a picture of the political conditions of the Egypt of his times from the scanty contemporary monuments." The translation is adequate, though marred here and there by a too literal rendering.

M. A. MURRAY.

Italy: Archæology. Randall-MacIver.

*The Iron Age in Italy: a Study of those Aspects of the Early Civilisations which are neither Villanovan nor Etruscan.* By D. Randall-MacIver. Oxford: At the Clarendon Press, 1927. Pp. xv, 243; v, with 47 plates and 90 figures in the text. Price 84s.

This work is a companion volume to the same author's "Villanovans and Early Etruscans," published by the Clarendon Press in 1924, and enjoys the advantages of an equally luxurious and admirable format. Its subject is a survey of the archæological provinces of Italy in the Iron Age,

other than those of Bologna, Etruria, and Latium. The first section treats of Northern Italy, a district that is conveniently demarkated as an archæological unity by the north to south Rimini-Rome line that divides the cremation-burials of the Villanovans, Atestines, and others, from the inhumation-burials of the centre. The Atestine civilisation is described in detail, and Dr. Randall-MacIver is able to approve, with certain reservations, the usual chronology of the graves in the Este cemeteries. Este I, he would remind us, depends on a single tomb, and is accordingly a period of dubious significance; Este II, divided by the author into four stages, covers the period 950-500 B.C.; Este III, characterised by the splendid bronze situlæ and girdles, is the period 500-350 B.C.; Este IV extends over the next century and a half, and is a period wherein it is possible to discern a Gaulish influence—though this, unhappily for those who love Gaul, is not described in the present work. The reader will note with great interest the author's conviction that the famous situlæ of North Italy (the Certosa situla is here most magnificently illustrated) are to be attributed to Etruscan influence, and his opinion, adjoined as corollary to this view, that the Hallstatt situla-lid and the decorated bronzes from Watsch in Carniola, here said to be later in date than 450 B.C., are debased copies of Etruscan originals. Moreover, it is just conceivable, according to the author's reasoning, that these Alpine bronzes were made in Este.

The account of the civilisation of the Picenes in Central Italy is likely to be, as Dr. Randall-MacIver warns us in the preface, a novelty and surprise. For the inexpert reader the surprise is, of course, a pleasant one, as the author gives a lucid and convenient summary of a rich material, as yet almost unknown to English students, by the expedient of treating it as the product of a single, albeit complex, archæological culture. It must be left to those with a knowledge of early Italian archæology equalling that of the author to challenge, as they most assuredly will, the invention of a Picene folk who, being entirely independent of the Villanovans, were long-established in the land and sprung from the ancient Neolithic stock; for this single Picene culture, in the author's view, includes those elements hitherto believed to represent an invasion from across the Adriatic that was relatively late in date.

The third section of the book deals with Southern Italy, that is the provinces of Sicily, Campania, Calabria, and Apulia, but here an insufficiency of material prevents

the author from exercising his customary treatment according to large ethnic groups, and he finds himself able only to give short studies of the isolated problems presented by the few finds available. Perhaps it need only be noted here that Dr. Randall-MacIver insists with some force that the Siculan civilisation was almost completely independent of Italian influence before the period of the Greek colonisation.

It is unnecessary to tell those who already know Dr. Randall-MacIver's work, that this big book makes pleasant reading, and is a welcome and refreshing volume for those who are lost in that small but thorny wilderness, the article "*Italien B*" in Ebert's "*Reallexikon*." In fact, it might be urged as a general criticism against this book that Dr. Randall-MacIver proceeds too smoothly in his course, and that we hear less than is good for us of the perplexing counter-theories and confusions that embarrass the other enquirers struggling along the same difficult journey. It is a duty, therefore, to state plainly that theory and interpretation are not Dr. MacIver's chosen province in these two great volumes, and that all criticism must be silenced when his work is considered as the fulfilment of a gigantic and comprehensive essay in regional survey. Dr. Randall-MacIver has brought the museums of Italy to our desks, and our gratitude to him for this great service must not be measured in ordinary terms.

T. D. KENDRICK.

India: Archæology.

Mitra.

*Prehistoric India*. Second Edition.

By P. Mitra. Calcutta University.

1927. Pp. xxviii x 512. 52 plates.

Little is known of Indian prehistory, less has been published, and most of the published material relates to work done three generations ago. In only two sites have palæoliths been found associated with an extinct fauna; they yielded each one artefact. Of later palæolithic forms, one chert *burin* is figured by Foote; some bones and teeth "of Magdalenian aspect" found in a Kurnool cave were promptly lost. No neolithic settlement has been excavated; only one bronze celt is recorded (in 1874); it was "never figured and soon lost." Apart from the recent work of Dr. Hunt and a few others, the records of megalithic excavations, except Breeks' Nilgiri monograph and Logan's Malabar, are defective. A full account of Harappa and Mohenjo-Daro is yet to come. In the dearth of evidence pre-historic India affords wide scope for the imagination. There is hardly a phase of early culture with which Mr. Mitra fails to find some Indian analogy; Africa, America and Australasia, too, are laid under contribu-

tion. Some of his conclusions may be right; the Mousterian and Aurignacian types noted by him in the Indian Museum collection should be figured, and the find spots examined afresh; the cave paintings and rock carvings deserve serious consideration. But few will accept Mr. Mitra's "culture sequences and origins" on the evidence he cites. One artefact does not make a culture, and the typology of surface finds does not convince. In archæology the spade is mightier than the pen, and in the absence of stratified evidence, properly figured and described, it is unsafe to generalise. In his effort to fit the disjointed Indian evidence into the steel frame of European chronology, Mr. Mitra is attempting the impossible. F. J. R.

Persia: Archæology.

Huart.

*Ancient Persia and Iranian Civilisation*. By Clement Huart. 92 (History of Civilisation Series.) London: Kegan Paul & Co. 1927. Pp. 232. Price 12s. 6d.

Mons. Clement Huart has performed a useful service for those who, without desiring to acquire a specialist's knowledge of the Early History of Persia, must know something of the development of its culture and organisation in order to preserve their sense of perspective. He deals with the spread of Hellenism in the East under the Seleucids, the Parthian revival, and the great age of the Sassanids. Especially valuable are the chapters dealing with Mithraism and the form in which elements of it survive in Mazdaism to-day.

E. N. F.

Anthropology: Social.

Heilborn.

*The Opposite Sexes. A Study of Woman's Natural and Cultural History*. By Dr. Adolf Heilborn. Translated by J. E. Pryde-Hughes. London: Methuen & Co. 1927. Pp. 152. Price 6s. 93

This little book is divided into three chapters, which are concerned respectively with the physical structure and the soul of woman and the development of her social position. After pointing out that the anatomist Albrecht once tried to prove the "greater bestiality" of woman, the author agrees with Havelock Ellis that woman represents a more infantile and therefore less specialised and in that sense higher type than man. The fundamental character of the sex difference, extending to "finger tips and toes," is emphasised, and the secondary sexual characters as resulting from the generative glands, but the deep metabolic difference expressing itself in every part and every activity of the organism is, no doubt, a result of the difference in the chromosome

constitution of every cell in the body. This most fundamental difference, from which the others emanate, is not mentioned at all.

Among the anatomical features of woman a long index finger is cited, which Pfitzner attributed to the female habit of gesticulation! More certain skeletal features are the wider pelvis, the right angle made by the condyle with the shaft of the thigh bone, the shorter stature and smaller, thinner-boned, rounder skull, less removed from that of the child. Further, while the coccyx in man numbers five bones, in woman it contains four, and in the orang and gibbon three. The cranial capacity averages 1,300 c.c. for the European female, as against 1,450 c.c. for the male. The smaller head of the female child at birth is chiefly due to the smaller brain, which averages 120 grams less at birth and is much less convoluted. The muscles and blood of woman have a higher water-content than in man, and subcutaneous fatty tissue gives a more rounded form. The blood is richer in plasma, but contains far fewer red corpuscles and thereby a less ferruginous content.

The author supports E. Fischer's view that many human racial characters, such as steatopygia and the permanent breast, are signs of domestication, corresponding to features found in domestic animals. It is claimed that domestic animals, requiring less alertness, have smaller brains than their wild ancestors. In general, the lower a people stands culturally, the smaller are the differences between the sexes.

The other chapters deal with such topics as mother right among primitive peoples, the Hetairai, prostitution, Mariolatry and monogamy in their historical development.

R. RUGGLES GATES.

Africa. Technology. Lindblom.

*The Use of Stilts, especially in Africa and America.* By Dr. K. G. Lindblom. Stockholm: Riksmuseets Etnografiska Avdelning. 1927. Pp. 40, with 6 plates. **94**

Dr. Lindblom has followed up his interesting "Die Schleuder in Afrika und anderwärts" with a monograph on stilts

which, though short, collects in a convenient form the available data on the subject. A map given on p. 24 graphically shows their recorded distribution throughout Africa. There are strong grounds for predicating a ritual origin for stilts, and further investigation would in all likelihood prove their close dependence on initiation rites. Their correlation with masks, however, is not nearly so complete as is suggested by the author, and there is nothing to be gained by assuming that these two elements in initiation rituals must always be found together. J. H. D.

America, South: Ethnography. McGovern.

*Jungle Paths and Inca Ruins.* By William Montgomery McGovern, D.Ph. (Oxon.). London: Hutchinson & Co. 1927. Pp. 381. Price 21s. net. **95**

In this book, Dr. McGovern gives a popular and lively account of his journey up the Amazon and of his expedition to Peru. Although the book contains little that can be regarded as serviceable to the anthropologist, he tells us sufficient of little-known Indian tribes to hope that he will publish later a scientific account of the peoples with whom he came into contact. E. N. F.

India: Religion. Thomas.

*The Life of Buddha.* By Edward T. Thomas. (History of Civilization Series.) London: Kegan Paul & Co. 1927. Pp. 288. Price 12s. 6d. **96**

This informative account of the life of Buddha by a scholar whose acquaintance with the literature of his subject is unrivalled, is valuable in that it incorporates the results of much careful and critical study by such scholars as Rhys Davies, Cairn, and Oldenburg, which has not previously been utilised in books of this character. Mr. Thomas has gone further and has placed before his readers an appreciation of the extent to which their work has modified previous views. As in so many other volumes of this series, its special merit lies in the breadth of view which brings the subjects into relation with the general stream of development of civilisation. E. N. F.

## CORRESPONDENCE.

Melanesia: Sociology.

Barnard.

**The Regulation of Marriage in Ambrim.**

To the Editor of MAN.

SIR,—There is one curious mistake in Mr. Deacon's paper on the Regulation of Marriage in Ambrim (*J.R.A.I.*, Vol. LVII, pp. 325-342), that though it is of minor importance is worth immediate correction. He gives the Ranon term for

brother (w.s.) as *Mweneg Re*. He thus makes it appear that the root word is *Re*, with a similar prefix to the words for husband and wife. Now in the Balap system the word for brother is *munukuli*. I have a number of Ambrim kinship systems, collected by Rivers or myself, from which it is possible to trace the phonetic changes in the different dialects. In Paama the term is *manu-kuli*, at Port



Vato *mene-kuli*. In two systems, collected by Dr. Rivers and myself from the neighbourhood of Balap, it is *munu-kuli*. In West Ambrim where "l" is changed to "r," it appears as *mene-kari* and *mainye-kari*. In a system from Onal to the north of Ranon, Rivers has it as *mani-nger* or *mani-geri*, if we are to adopt with Deacon the Presbyterian Mission orthography in which "ng" is written "g." There can be no doubt that the Ranon term should be written *mwene-ngere* or *mwene-ng're*. The root MANE in various forms is a very widely distributed term for brother (w.s.), being found in Tanna, Eromanga, Epi, Malekula, west and north-east Santo. The suffix *-kuli*, *-kari*, *-nger* is peculiar to the Ambrim and Paama languages. According to Rivers, the "k," "ng," is the first possessive suffix which leaves *-uli*, or *-ari* as the peculiar additional suffix. I believe it was described by Rivers as a "suffix of endearment." It seems to be used everywhere for similar relatives, brother to sister, sister to brother, and for the grandchildren. It is seen also in the Balap term for sister (m.s.), *vevenu-kuli*, *vevenu* being a form of a widely distributed root, compare *vavine*, Banks Islands, and the contracted *iwunye* of Ranon.

Mr. Sidney Ray is surely wrong in saying that the primary meaning of the word *sog* is "my companion," and treating it as though it were a substantival derivative of the *sawa* group (*J.R.A.I.*, Vol. LVII, p. 368, note 1). It is true that substantives from this root are found in the New Hebrides. But in West Ambrim, in Balap and Sulol, *sog* or *sok* is a possessive noun that usually precedes but may follow the noun governed. Dr. Rivers went into the question of the use of the possessive in Ambrim in some detail, not in relation to the kinship terms but in regard to the use for objects that were personal or communal property. In the south-western dialects, Port Vato, Susuvi, Balap, etc., both Rivers and I have written it *sok*: in the Sulol dialect Rivers writes it both *sok* and *sak*. In Lonwolwol, to the north of Sulol, where "s" is changed to "h," *sak* appears as *hak*. Unfortunately I have not a copy of all Rivers's evidence on the use of these words. Rivers says: "The nature of the pronoun used for property depends on whether the property is communal or individual. Thus it is customary to speak of 'our club,' *ha memchal puer*, and 'our spear,' *ha memchal meta*, but of 'my bow,' *wi sak*, 'or of 'my sling,' *hak hesesever*. A big garden is 'our garden,' *sa memchal tel*, 'but a man who has made a small garden for himself might speak of *sok* (sic) *tel*.'"

The final "g" or "k" of *so-g*, *ha-k*, etc., is the first person possessive suffix and *so*, *sa*, or *ha* is the root of the possessive noun. Unfortunately the examples that I copied from Rivers's notes are not localised as to dialect, but the *sa-memchal* and *ha-memchal* must be *sa* in Sulol, *ha* in Lonwolwol. In Balap, *sog* precedes the terms for husband and for wife (=my woman), and also the term *terian*. At Sulol and Lonwolwol it is used with these terms, and also with the very different term for sister (m.s.) and with an alternative term *metelo* used for the sister's child.

The precedent possessive *mwene* seems to replace *sak* in the . . . . . It appears in the Onal system as *maning*, used with the terms for husband and wife, and in the same form and with the same terms in an incomplete system I obtained from the neighbourhood of Ranon. In Codrington's "Melanesian Languages," there are notes on a north Ambrim dialect from the Ranon-Onal coast. In this *mwene*, or as Codrington writes it, *mene* (m = mw), is given as a possessive noun. Thus *mwene*ng *viria*, "my land," *mwene*am *ul*, "thy garment," *mwene*en *were*, "his place," etc.

Perhaps while I am dealing with this subject I might also point out that *tivy*u and *vavu* are variants of the term for grandparents (of all kinds), which occur at Onal in the more usual form of *tubu*, and that similarly *membyu* and *maryu* are variants of the word for grandchild (compare the *mabi* of Pentecost, etc.), and any attempt to find the key meanings of these terms must take account of their distribution and meaning elsewhere. But to a detailed discussion of Mrs. Seligman's analysis of Ambrim society I hope to return later.

I remain, Sir,

Yours faithfully,

T. T. BARNARD.

Cape Town,  
April 27, 1928.

Africa: Ethnography.  
**Noose Traps.**

Migeod.

To the Editor of MAN.

98

SIR,—With reference to Mr. Lindblom's article, MAN, 1928, 69, the Mambila, located in British Cameroons, lat. 7 N., long. 11 E., similarly use great nets at the top of grassland valleys to catch flying birds.

I referred to this in my book "Through British Cameroons," 1925, page 157.

As to signalling bells, fishermen in the Benue river, Nigeria, use metal bells on their rods. When the bell rings they wake up and take the fish.

Yours, etc.,

F. W. H. MIGEOD.



Britain: Archæology.

Philip.

**Deneholes.***To the Editor of MAN.*

99

SIR,—I read Mr. Newton's article on "Deneholes" with very much interest. There is quite a lot in it I do not agree with and quite a lot, I am sure, others would oppose. I am only writing for the benefit of those who are not familiar with the subject to suggest that they do not make up their minds without having studied the subject a little more.

Yours faithfully,

ALEX. J. PHILIP.

New Guinea: Ethnology.

Murray.

**"The Children of the Sun."***To the Editor of MAN.*

100

SIR,—Quite two years ago I wrote you a letter seeking enlightenment on certain matters (and especially a map) which I had found puzzling in Mr. Perry's "Children of the Sun."

My letter appeared in MAN of June, 1926, and Professor Perry kindly published an answer in a later number. The answer did not seem to me to carry the matter any further, so I did not reply.

And now, in November, 1927, comes a letter from Mr. Chinnery in response to a reference made to him by Mr. Perry.

The thing has dragged on for such an unconscionable time that I hardly like to "take the floor" again, especially as the delay is entirely my fault, for it is caused by my living in so outlandish a place as Papua.

But, after some doubt, and in response to Mr. Chinnery's invitation, I am writing to say that I did not see his letter until to-day, and that I have never seen the map which he contributed to the *J.R.A.I.*, or read the article which it was intended to illustrate.

Consequently, I can pass no judgment on either of them. The two maps (that

is, Mr. Chinnery's and Mr. Perry's) were apparently noted differently and for different purposes—so I gather from Mr. Chinnery's letter—and I cannot see that the difficulties presented by Mr. Perry's map are lessened by the fact that Mr. Chinnery once drew a similar map to illustrate a different subject matter.

Mr. Chinnery agrees with me about the Trobriands. He says that no "archaic" remains have been found there, and that is what I said. But I went on to say that pearls have been found there, and to ask why Mr. Perry's map, which shows pearls elsewhere in Papua, does not show them in the Trobriands. Many thousands of pounds' worth of pearls have been sold from the Trobriands, and, I think, none from anywhere else in the Territory. Mr. Chinnery knows this as well as I, but (if I understand him aright) his article had nothing to do with pearls, and therefore, naturally, he did not mark them on his map either at the Trobriands or anywhere else. What puzzled me in Mr. Perry's map was, not that it does not show pearls, but that it shows them in the wrong places.

So Mr. Chinnery and I agree about the "archaic remains," and I am sure that we also agree about the pearls, in the Trobriands. And I have no doubt that we agree about the "polished stone implements," and that Mr. Chinnery knows as well as I that "they are found all over Papua except so far as they have been superseded by steel," and that "they are still manufactured occasionally."

Therefore, it seems that, in the end, there is little or nothing left for Mr. Chinnery and myself to differ about; and, indeed, it would be surprising if there were much difference of opinion between us, for we served together in Papua for many years, and the facts which I have mentioned must be as familiar to him as to myself.

Yours faithfully,

J. H. P. MURRAY.

**ANTHROPOLOGICAL NOTES.**

*Prizes for Research.*—An offer of considerable interest to anthropologists and statisticians is made by the Eugenics Research Association, which announces a competition with prizes of \$1,000 and \$200 for the two best essays by American authors on "A Comparison of both the crude birth rate per 1,000 females 15 to 45 years of age, and the "vital index" (or  $\frac{100 \text{ birth}}{\text{deaths}}$  ratio) of the Nordic and Non-Nordic peoples in the Americas." Data are to be considered in different periods from 1850 to the present time or last available census. Nordic

101

peoples are considered to be those whose ancestors came mainly from Nordic countries. Such countries are defined as including the Scandinavian countries south of Lat. 63° N., the Netherlands, England, Scotland, North Ireland, and certain German states, non-Nordic being regarded as the rest of Europe, Asia and Africa north of the Zambesi. Essays must reach the Association in Cold Spring Harbor, by February 1st, 1929. Prizes of the same amount are offered to European authors for essays on the same subject and under similar conditions.



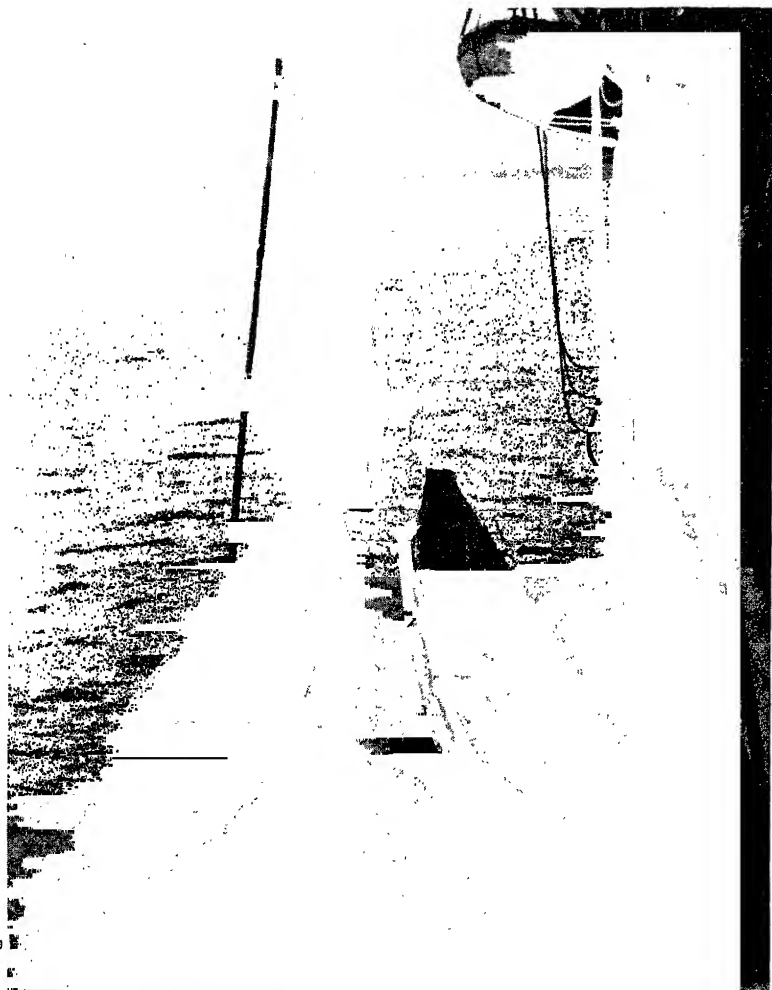


FIG. 1.—TWO SEA-GOING COLOMBIAN BALANCED CANOES.



FIG. 2.—A FERRY-CANOE AT CHARCO.

SOUTH AMERICAN BALANCED CANOES.

## ORIGINAL ARTICLES.

With Plate H.

America, South: Technology.

Hornell.

**South American Balanced Canoes: Stages in the Invention of the Double Outrigger.** *By James Hornell, F.L.S.* **102**

During the *St. George* Expedition to the Pacific in 1924, whilst the ship lay off the island of Gorgona, situated some 20 miles distant from the Pacific coast of Colombia (S.A.), canoes sailed over from the mainland almost daily; the majority came for fishing, some to attend to a grazing property under development at the south end of the island, whilst two brought Customs officers to learn what business a strange ship had at this lonely island. All were of identical type, that illustrated in plate H, Fig. 1.

The hull of these canoes is a very gracefully modelled dug-out with great overhang at each end, due to the provision of a wide terminal platform both fore and aft, made possible by the length and gentleness of the curve between the horizontal bottom and each extremity. The forward part of the hull has considerable and graceful sheer, the after end rather less; hence the forward platform is higher and more emphasised than the hinder one. Both extremities are truncate, but whereas the fore margin of the forward platform is carved into the form of a downturned scroll, distinctly seen when viewed from one side, the aft end finishes off in a simple straightcut transverse truncation. The bottom is rounded, without the vestige of a keel.

No strengthening ribs are provided within the hull, the inner surface being given a beautifully smooth finish. No thwarts are found in the smaller canoes, save for a thin rod which is placed near each end about midway between the bottom and the gunwale; the ends are inserted into small holes in the sides of the canoe. In larger ones a true thwart, broad and flat, is provided well aft, laid upon a wooden cleat at each side a few inches below the gunwale. In those where a mast is used, well forward of amidships, a board is lashed athwart the gunwales, with a hole in the centre to take the mast. This mast-thwart is termed the *taburete*.

A hole perforates the centre of the forward platform, and through this is passed a thin and beautifully-spun black cord, fine like a salmon-fishing line, which is all that the crew have for a painter.

The dimensions of these canoes vary considerably, particularly the length. A medium-sized hull was found to measure as follows: length overall,  $18\frac{1}{2}$  feet; beam amidships (outside), 25 inches; depth,  $13\frac{1}{2}$  inches; thickness of side at gunwale,  $\frac{7}{8}$  inch; width of end platforms at outer end, 14 inches; length to point of excavation of hull, 15 inches.

Paddles are used for steering and for propulsion when the sail is not in use; they are of elegant form, with a broadly lanceolate blade and a horned cross-bar at the butt end of the loom.

Each canoe has a wooden buoy (*boya*) and a stone-loaded "killick" when engaged in fishing off the coast. The *boya* is a short, stout, cylinder of balsa wood, tapered slightly to a semi-spherical knob at the fore end, while behind it is truncate. At mid-length is an encircling groove. The buoy "rope" is the usual thin black cord. The attached end is secured around the midlength groove and then carried to the forward end, there tied around the neck behind the terminal knob, the slack being wound around the cylindrical butt. The anchor is a typical killick. It is formed of two stout wooden arms, pointed at the ends and crossed over one another at mid-length, where they are nailed in position. These crossed arms are weighted by a heavy stone enclosed within a cage of four withy rods; these rods are chosen from branches having a thick node and are cut so that this node

forms one end; each is passed through a hole in the middle of one of the four flukes from without till arrested by the knob of the node. The four distal rod ends are then brought together above the weight stone and lashed together, so forming a cage for the weight. To secure the stone still further, a fibre lashing is passed around the stone and secured to each of the rods in turn. The size of a typical killick is: height, 14 inches; length of each fluke, 8 inches; length of stone,  $7\frac{1}{2}$  inch.

A bailer (*tortuma*) is found in every canoe. It consists of half a small calabash. This must always be kept mouth up, to ensure good luck; to be placed wrong way up might entail the wreck of the canoe.

*Rig.*—These canoes are sprit-rigged. The sprit sail used is low in height and wide fore and aft; otherwise the form is typical. Being of low height, the mast conforms to this. The latter is a solid pole, stepped so as to rake sharply aft; the other spars are of bamboo. The sail is of cotton and untanned. In a typical canoe the dimensions were: height of mast, 11 feet; length of sprit, 12 feet; of boom,  $18\frac{1}{2}$  feet. The sprit butt crosses the mast about 3 feet above its heel.

*The outriggered balance fittings.*—With one exception only, the canoes which visited us at Gorgona possessed a device of the greatest interest in the comparative study of the origin and evolution of the outrigger canoe, being provided with a balance log lashed outboard to each side (Fig. 1). All boats so equipped had low

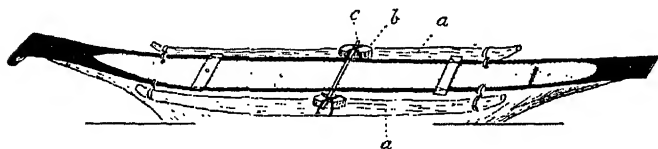


FIG. 1.

freeboard; in the solitary instance where no balance logs were present, the canoe had unusually high sides. Hence it appears that the use of this contrivance is a consequence

of the difficulty now experienced of obtaining tree trunks of sufficient diameter to admit of a high enough freeboard being possible. The balance logs are made from the extremely buoyant trunk of the *balsa* tree. The length of each is approximately just over one-half the total length of the canoe; the log, which is called simply *balsa*, is lashed to the side of the dugout by fibre or cord at points close to the fore and aft ends, the lashing passing in each case through a hole bored through the side of the canoe just below the edge. A median attachment is also invariably present, but here, instead of the *balsa* being fastened directly to the side of the canoe, it is lashed to the projecting end of a short pole (*barrote*) laid across the hull to which it is secured. The *balsa* is, however, not attached directly to the *barrote*, but has a chunk of *balsa* wood interposed in order to hold the middle of the *balsa* permanently depressed to the proper level. The intercalated piece of wood is the *tojin*; it is about 5 inches in length, 3 to 4 inches thick and is lenticular in horizontal section. *Guasca* is the name of the lashing between the *balsa* and the *barrote*. The latter is to be equated with the boom of an outrigger canoe.

During our stay at Gorgona, a visit to Charco, the nearest town on the adjacent mainland of Colombia and some 10 miles up a wide river, showed that the type we had become familiar with was the smallest of three existing types, and the first in a short series having the double outrigger as its final stage. At Charco, the further types seen were: (a), a ferry boat consisting of a specially large and roomy dugout, and (b) an *ibabura*, a small local coaster having built-up sides upon a dugout basis.

In the former (Plate H, Fig. 2) the *balsa* logs were not attached to the side of the canoe by lashing, but were tied in true outrigger style to the outboard ends of two stout outrigger booms, one fore and the other aft, laid athwart the hull. As these booms are made from straight bamboos, the *balsa* logs are depressed a few

inches below the gunwale by means of the intercalation of a small disc of wood between boom and float, the joint being securely lashed up by stout bark fibre straps. The booms in turn are tied to the gunwales by cord passing over the boom and through two holes bored through the side of the hull. Except that the distance of each float (the *balsa* log) from the side of the canoe is very short—about 8 inches—this arrangement is that of the typical outrigger canoe in almost its simplest form.

Of the *ibabura*, several examples were seen. A long dugout forms the basis of the hull; on this is superposed, according to size, either one, two or three plank strakes, flared outwards at a moderate angle. Within, the hull is strengthened and the strakes secured in position by a number of ribs spaced rather widely apart. The length ranges from 30 to 40 feet. The stern is markedly narrow and of transom form, built up of short boards connecting the after ends of the side strakes. The centre is perforated by a large hole, through which a long and powerful steering oar, of peculiar blade form, is passed when in use; this *grande remo* is termed *espadilla* in Spanish. The hull is open and wholly undecked. There is a single mast, but the form of sail was not ascertained. This kind of vessel is employed in the minor coasting trade between the ports of Western Colombia (Buenaventura, Tumaco, Charco, etc.) in the same way as the schooner-rigged *bongos* serve the coast of Panama. In this trade it is, however, being ousted rapidly by cutter or sloop-rigged plank-built coasters of European model, with greater beam and carrying capacity.

The *ibabura* appears to represent an older and more primitive type than the Panamanian *bongo*, though both have a big dugout as the essential part of their hull. But the washstrake in the *bongo* is vertical, and a schooner rig of European pattern has been adopted.

Although the present is, so far as I am aware, the first recorded instance of the occurrence of canoes with outriggered balanced logs on the coast of Colombia, they were noticed early in last century at Valparaiso in Chili. Paris, in his magnificent monograph\* figures (Pl. 130) one from this place, which, in essentials, is of the same type as the Colombian one. In the letterpress (p. 150) he says: "the only craft which are not European (in design) at Valparaiso are some canoes hollowed out of tree trunks, badly rounded and surmounted by obliquely-placed washstrakes." The plate shows one of these canoes both in side view and end on. As will be seen from Figs. 2 and 3, copied from his plate, a log of wood is tied to the side of the dugout just below its upper edge, as in the canoes seen at Gorgona; the form of the prow has also the same peculiar turned-down extremity found in the latter. The main difference is the addition of washboards, thus providing a stage intermediate between the Colombian dugout and the built-up *ibabura*. From this record it is proved that, a hundred years ago, the balanced canoe had a very extensive

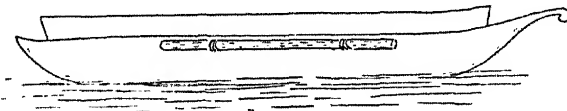


FIG. 2.

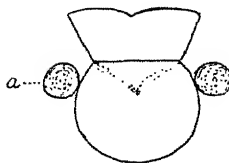


FIG. 3.

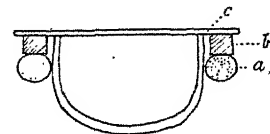


FIG. 4.

\* Paris, F. E., "Essai sur la construction navale des peuples Extra-Européens." Paris, 1841-1843.

range on the South American coast; for, if it occurred in Colombia and in Chili, we may reasonably presume that it was also known at intermediate points which linked up the two extremes. We come now to the problem of the origin of this peculiarly primitive type of outrigger; is it an independent invention of the coastal Amerinds, or is it the result of diffusion?

If the problem were not complicated by Spanish contact, I should unhesitatingly favour the former hypothesis. Apart from the Spaniards, the only people who could "diffuse" the knowledge of the outrigger canoe on the South American coast would be the Polynesians. Now, this race must have outgrown the double outrigger at a very early stage in their migrations into the South Sea, for the double form is far less seaworthy than the single; besides, with a few very doubtful exceptions, no Polynesians have been known to make use of the double canoe within historic times. A further objection to the Polynesian origin of the American outrigger is the fact that the general design of the latter is cruder and less serviceable than the simplest and rudest of Polynesian ones, for in every Polynesian outrigger the float is boomed out to a considerable distance and *always* rests upon the surface of the water under normal conditions. In the American design the evolution of the outrigger has, in fact, not progressed sufficiently to constitute it a really typical outrigger; in its highest development it has not passed completely out of the balance-log stage, for the float is not ordinarily immersed, coming into action only when the canoe heels over considerably.

When we come to consider the possibility of Spanish contact being the active agent in diffusion, we find what appears to be strong presumptive evidence in its favour.

By reason of the fortuitous choice by Magellan of a northern latitude for the crossing of the Pacific, and the virtual restriction of effective Spanish power in Oceania to the Philippines, her navigators missed discovering Polynesia; thus it happened that the thousands of Spaniards who voyaged across the Pacific in the golden days of the Manilla-Acapulco treasure ships, knew little or nothing of outriggers save what they saw in Manilla harbour. And these were (and are) of a type distinct from those of Polynesia, all being double outriggers. There existed, however, side by side with them a class of canoe of different type, used extensively for passenger and light cargo traffic on the river and the harbour of Manilla. These are described by Paris (*loc. cit.*, p. 59) as having a dugout hull, 5-7 metres long, 0.72 metre wide, and 0.555 metre deep. He says: "It would have little stability without two great bamboos attached to the sides, a little above the water, which, supporting it when it heels over hinder it from capsizing and prevent water from entering when the sea is a little rough, the freeboard being low." It carries three or four passengers, and "every day there arrive a fleet of them loaded with market supplies. Those that are used only on rivers and lagoons are 10-12 metres long, but have no greater width than the others." From his illustration (Pl. 75), it is seen that the bamboos are tied directly to the sides of the dugout, and not to booms. The name given by Paris to this Manilla craft is *banka*.

Is it a mere coincidence that the small schooner-rigged market craft of Panama are called *bongo*?

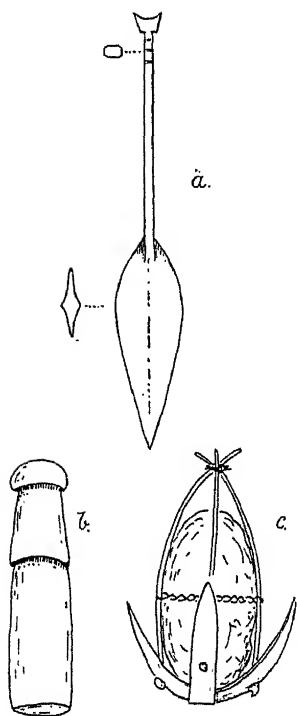


FIG. 5.

The structure described above being practically identical with that of the smallest of the Colombian canoes, the inference is forced upon us that the addition of balance logs to the indigenous Indian dugout has been due to the initiative of Spanish settlers who, as seamen in the Manila trade, had seen the utility of the contrivance during their visits to the Philippine Islands. It is probable that the device, as originally introduced, consisted of the attachment of a *balsa* log directly to each side of a dugout. The advanced stage seen in the ferry boat and the *ibabura*, where the logs are boomed out some little distance on each side, would appear to be an improvement devised on the American coast, one that goes far towards what, if carried to its logical conclusion, would give us an instance of the independent invention of the double outrigger. As it is, the very imperfection of the design, and the incompleteness of the invention, is of extremely great importance to us, for it is the only instance known that furnishes direct evidence towards the elucidation of the problem of the origin and invention of double outrigger canoes. It proves that, in one part of the world, the double outrigger has developed, or rather is developing, from balance logs tied to the upper part of the sides of dugout canoes. That double outriggers in other regions have had a similar origin and development is probable, but does not necessarily follow.

JAMES HORNELL.

#### DESCRIPTION OF THE ILLUSTRATIONS.

##### Plate H.

Fig. 1.—Two sea-going Colombian balanced canoes, alongside the *St. George* at Gorgona Island.

Fig. 2.—A ferry-canoe at Charco. In this the *balsa* balance logs are attached at a distance outboard of 8 inches to the ends of two stout booms athwart the canoe.

##### Text Figures.

Fig. 1.—Sketch of a small Colombian canoe with a balance log attached to each side. (a) the logs; (b) the *tojin*, a lenticular piece of *balsa* wood intercalated between each outboard end of a slender median "boom" (c) and its respective balance log.

Fig. 2.—Side view of a Valparaíso canoe, after Paris (early nineteenth century).

Fig. 3.—End view of the same, also after Paris. (a) the balance logs.

Fig. 4.—Diagrammatic transverse section through a Colombian balanced canoe at mid length. (a) the *balsa* logs; (b) the *tojin*; (c) the boom.

Fig. 5.—The fittings of a Colombian canoe. (a) a paddle; (b) a buoy; (c) the killick, weighted with a large rounded stone.

#### Melanesia: Sociology.

Barnard.

#### The Social Organisation of Ambrim. By Professor T. T. Barnard.

The recent publication of the late Mr. Deacon's paper on the regulation of marriage in Ambrym (*J.R.A.I.*, Vol. LVII, July-December, 1927) has disclosed the existence of an unique type of social organisation of extreme interest and importance. Both Dr. Haddon and Mr. Deacon have mentioned me as the possessor of unpublished material relating to this area, but no reference whatsoever is made to Rivers and his published statements on the social organisation of this island. Since Ambrim is given some prominence in nearly all Rivers's later works and his statements thereon are not reconcilable with Deacon's account, I feel that the following explanation of the discrepancy and the history of my connection with Ambrim may be of interest.

During the autumn and winter of 1914, Rivers spent four or five months in the New Hebrides. He returned to England early in 1915, with a mass of new material from areas that he had been unable to visit during his previous expedition, and especially from the large islands of Espiritu Santo and Ambrim. After his return, Rivers published three short accounts of his general conclusions, "Melanesian



Gerontocracy,"\* "Descent and Ceremonial in Ambrim,"† and the article "New Hebrides" in Hasting's "Encyclopædia."‡ From these accounts and from the fairly frequent references to Ambrim in his lectures at Cambridge after the war (*vide* "Social Organisation," *passim*), it appeared that in Ambrim, contrary to Rivers's expectations, society was patrilineal, and based on exogamous villages. The kinship system showed the "grandmother" marriage features, such as Rivers had found in Fiji and in the Solomons, and there was evidence that actual marriage with the father's father's widow was practised. Other anomalous marriages also occurred and were said to be reflected in the kinship system. No general account of the system had, however, been published, nor did any mention of its special features occur in the lectures, as far as I can recollect. A further peculiarity of Ambrim society was the presence within each village of a number of *vantinbūl*; these were kinship groups apparently of a bilateral nature chiefly concerned with the ownership of land and other property: in general, they seemed to be patrilineal joint families—to use Rivers's own term—to which, however, the sister's children were attached by the recognition of certain rights in land.

In 1922 I decided, at Rivers's suggestion, to go to the New Hebrides. He recommended that I should devote my time to one of three special areas that he wished to have investigated: the Southern Banks Islands, Pentecost (Raga), or Northern Santo. He promised to show me some of his material from Santo, but his sudden death prevented this promise being fulfilled. I left for the islands in July, 1922, in almost complete ignorance of the scope and nature of his unpublished work.

Ambrim lay outside the region of my research, and I should not have brought back any information whatsoever from this island but for the fact that I became interested in the distribution of kinship terms, and both at Vila and in Santo I took every opportunity of obtaining pedigrees and kinship systems from men of other islands. In this way I collected small pedigrees from five Ambrim men. In each case I was working in Pidgin English, with isolated and often unsatisfactory informants, with no opportunity for confirmation or even for further interviews. The information thus obtained was meagre and unsatisfactory. I was able to confirm the village exogamy and the "grandmother" terminology, but into the problems of the land-owning group, the *vantinbūl*, I did not trouble to inquire.

I returned to England with fairly full accounts of the social organisation of the Banks Islands, of Efate (Vate), and of certain areas in Northern Santo, and some thirty incomplete kinship systems, with notes on the general social organisation from other islands. In order to supplement this material, I wrote to Professor Elliot Smith, asking if I might see Rivers's unpublished notes, as I hoped therewith to be able to complete a comprehensive general survey of the regulation of marriage in the New Hebrides. Professor Elliot Smith kindly gave me permission to make whatever use I liked of Rivers's notes for this purpose.

The manuscript of the 1914 expedition consisted of twelve field note-books, which had been deposited in the Library of the Ethnological Museum at Cambridge, and certain typed sections on special cultural aspects that had been written up by Rivers before his death. In addition, I found at a somewhat later date a series of letters from Mr. Bowie, of the Presbyterian Mission, full of additional information and corrections, chiefly relating to Ambrim.

All Rivers's Ambrim material seemed to have been obtained at the Presbyterian Mission School at Tangoa, a small island off the south coast of Espiritu Santo. His two chief informants were William Temar and Lau, two teachers at the school from neighbouring villages in Western Ambrim, near Craig's Cove. He was assisted

\* *Man*, 1915.† *J.R.A.I.*, 1915.

‡ 1917.

throughout by Mr. Bowie, formerly a missionary in Ambrim, and at that time head of the school on Tangoa.

My experiences in the field, and especially in those areas in which Rivers had also worked, had convinced me that his methods were often superficial and had led me to a very different conception of Melanesian society as it exists in the New Hebrides, and also of its probable "history." The divergence of opinion was not, however, fundamental until I came to the consideration of Ambrim. The material from this island was extraordinarily detailed; and yet Rivers's treatment of his information seemed to me more than usually unsatisfactory. I was becoming more and more convinced that some form of class system had existed in Santo and the central islands and possibly further north. Ambrim seemed to me to be an area in which it was most clearly indicated, the grouping of alternate generations in the kinship system, which Rivers attributed to the grandmother marriage, being strongly indicative of "classes." As, however, there was no definite evidence of the existence of classes in Rivers's material, I was constrained to believe that the system had disappeared. In searching through Rivers's papers, however, I found Mr. Bowie's letters. One of these was written in answer to some article, perhaps "Descent and Ceremonial in Ambrim," that Rivers had sent him. Most of the information had been supplied by William and the definition of the *vantinbül*, about the exact nature of which there had always been some doubt, was based on William's statements. Mr. Bowie said in his letter: "I am sorry there should be so much in your MS. that Lau and the others say is wrong. Lau is positive that the conditions at his village (Lonwolwol) are the same as at William's (Sulol); and that the meanings of the kinship terms are the same at both places. Personally I am sure that Lau is right." Old William had died soon after Rivers's departure from the islands; all the additional information and corrections forwarded by Mr. Bowie were supplied by Lau and other Ambrim men at the school. Mr. Bowie continued: "Lau and the others here differ entirely from William about the *vantinbül*. For more than one reason I am certain that William's statements are wrong. The *vantinbül* are all those who call one another brother and sister, including the father's father and his brothers and sisters and the son's children. There are only two *vantinbül* in Lonwolwol composed of members of alternate generations." Mr. Bowie enclosed a copy of Lau's extensive pedigree marking the members of his *vantinbül*, the alternate generations in the male line in Lau's village, and one man distantly related to Lau and a member of another village (actually his F.F.Sis.D.D.S.). From this information it was obvious that Rivers's statements concerning the *vantinbül* were based on a misconception and that some class system was actually in existence in Ambrim. Using the large pedigree material available, I was able to work out, and prove to my own satisfaction at least, that Ambrim society was an example of a hitherto unsuspected class system with six classes. It must be remembered that no mention of the number six was made by Mr. Bowie, and the evidence available was entirely collected by Rivers. My own Ambrim material contained no information about the *vantinbül*. Without further corroboration, I have hesitated to publish, in what must have been a very lengthy and circumlocutory form, my refutation of Rivers's published statements from a reconsideration of Rivers's own evidence. My theory has been known both to Dr. Haddon and to Mrs. Seligman since the completion of my thesis, and I have made use of the Ambrim six-class system in my lectures both at Oxford and at Cape Town.

The publication of Mr. Deacon's admirable account of the regulation of marriage in Ambrim completely confirms my views. Sulol and Lunwolwol resemble Balap rather than Ranon; and the recognition of the cycle, the *batatun*, at Ranon is a new feature that, though implicit in the western systems, could not be deduced

from Rivers's material. Much of Rivers's material concerned the ritual of the men's associations; I made no use of this. All that I possess now is the draft of my thesis and various extracts and notes on the social organisation that I made while I was going through the original manuscripts. I hope, with Professor Elliot Smith's permission, to be able to publish shortly an account of the West Ambrim systems, with such additional information as was collected by Rivers or myself from other districts. The material from Sulol and Lunwolwol is so full that it will serve to clear up many points that Deacon's account leaves uncertain, and to provide a very clear conception of this extremely interesting type of social organisation. With the corroboration that Deacon has afforded, my original treatment of my differences with Rivers can be considerably shortened; and the publication of this particular section of his unpublished material will help rather than hinder whoever may eventually undertake the preparation of the rest.

There is one point on which some uncertainty remains. There is no doubt that the inclusion of the sister's son in the *vantinbül* was a misinterpretation of one of William's statements. Otherwise William defined the term as a patrilineal "joint family"—that is, the couple of descendant classes in any one village. His own village of Sulol was a composite village made up of the remnants of at least three depopulated villages. All six classes were here represented, and marriage within the village was allowed—William being careful to point out that this was not a breach of village exogamy since the individuals were still held to belong to their original villages. From the composite nature of Sulol, Rivers derived the impression that there were typically several *vantinbül* in each village.

All Rivers's published statements on the communal ownership of land by the *vantinbül*\* were based, as far as I can recollect, on William's evidence. It seems probable that the land-owning group was not Lau's *vantinbül*, the class, but the section of Deacon's *bwelem* in each village, a patrilineal "joint family" as far as I can judge. On the other hand the discussion of property took place at a later date than the original definition of the *vantinbül* as a kinship group. This definition was, as Rivers has stated, open to doubt and there is a possibility that in his later evidence William was using the term in Lau's sense and that it is the class and not the joint family in each village that holds its garden land in common. A further difficulty is raised by the probable etymology of the term *vantinbül*. *Vantin* means men and *bül* seems to be connected with *bwelem* and *bwulim*. It is not easy to see how the prefixed *vantin*, which seems to give a personal in place of an abstract sense to the term, can provide a distinctive meaning and imply an alternate section or class within the *bwelem*. Is it possible that William was more correct in his use of the term than Lau despite the clarity of Lau's evidence and that *vantinbül* is actually the equivalent of *bwelem*, as etymologically it would seem to be? It will be noticed that no term is recorded from Balap for the class except "line," but that at Ranon the wholly different terms *wor* and *verachei* are found. Until the etymology and significance of the term *vantinbül* can be settled—and I have not the evidence by me to do this—it is impossible to reinterpret with certainty Rivers's statements on the ownership of land in Ambrim.

It may be asked, what possible explanation can there be of Rivers's failure to discover the six-class system in an area in which he did such detailed research? It is true that the definitions given of the *vantinbül* were contradictory and in one instance—the supposed inclusion of the sister's son—misleading. The composite nature of Sulol also obscured the typical alternating *vantinbül* that comprise the simple village. Even so, Rivers could hardly have failed to stumble upon the correct interpretation but for a very serious defect in his sociological method, that, as

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\* See especially Land Tenure in Melanesia, "Psychology and Ethnology."






I have always felt, invalidates much of his theoretical treatment. This defect was appreciated by Deacon and is commented upon by him at the commencement of his paper. In all Rivers's work his treatment of his sociological material was carried out in terms of marriages and kinship systems. For him the significant facts in Ambrim society were the presence of marriage with the grandmother and with the daughter of the cross-cousin and other anomalous forms and the "dependence" of the kinship system thereon. The *vantinbül* were studied not as kinship groups but as land-owning groups. For the social anthropologist the significant feature of the Ambrim society is the class, the *vantinbül* as a kinship group, which determines both the marriages and the pattern of the kinship system. Sometimes this neglect of the basic groupings of society may not have been serious; sometimes, as in Ambrim (and in the Banks Islands), it has invalidated much or all of his conclusions.

Dr. Haddon is not quite correct in stating that Deacon had heard of my conclusions indirectly. I had met him on several occasions in Cambridge and when he decided to go to Malekula, I both spoke and wrote to him about the problem of Ambrim, and asked him, if he had any opportunity to extend his researches, to bear Ambrim in mind. I did no more than mention the six-class theory to him and suggest to him that he should look at the copy of my thesis in the Cambridge Library. Whether he did so or not I do not know. I failed to keep in touch with him after he left England. Neither the information that I gave him nor any subsequent information that Dr. Haddon may have sent him\* about my inferences, in any way detracts from the excellence of what is rightly called his independent discovery of the system. He was working with men from different areas and his analysis of the system is entirely his own. I doubt indeed if he carried away from our conversations more than the bare knowledge that Ambrim was worth investigating and that it might contain a class system. If I had sent him, as I should have done, a copy of my Ambrim material to the islands, certain points that are still obscure might have been cleared up. But his paper is so excellent an account of the system that, with the promise of more of his material to come, I doubt if my omission outbalances the value of his independent corroboration. By his death, social anthropology has suffered a loss that it can ill afford. T. T. BARNARD.

### Anthropology, Physical.

Harrower.

**A Note on Right-handedness.** By Gordon Harrower, M.B., Ch.B., 104  
D.Sc., F.R.S.E.

To the observant person in the Far East, one of the most striking features is the large number of Asiatics, especially Chinese, who are left-handed. Professor Elliot Smith, in his excellent book, "Evolution of Man," raises the question with reference to the antiquity of predominant right-handedness. The matter was forcibly drawn to my memory while discussing with Dr. Lim Boon Keng, the renowned Chinese scholar, the significance of the Chinese script. I was very surprised to learn that the hieroglyphic for "right" was , being a compound of  = the symbol for mouth plus , the ancient character for hand. Similarly, the hieroglyphic for "left" was , the symbol  meaning work, plus the symbol for hand. It appears that in ancient times it was

\* I merely reminded Deacon that Barnard had suspected a six-class system in Ambrym, but did not send him any account of what Barnard had done.—A. C. Haddon.

customary for the Chinese to eat using the right hand, while work was performed with the left hand.

That the free use of the left hand was not a monopoly of the ancient Chinese is evidenced by the fact that the drawings and sculpture of the ancient Egyptians show the figures in many cases in the act of using or holding weapons of war in the left hand, the shield being held in the right hand. Excellent examples are illustrated in "Wonders of the Past," where on p. 24 is a reproduction of the sepulchral chamber of King Tutankhamen with the king holding a lotus-ringed cane (?) in the left hand. On p. 50 is a reproduction of a bas-relief in the Temple of Angkor, representing figures bearing the body of a serpent under their left arms, the reverse of what one would expect to find in modern times. Apparently the right hand was used in ancient Greece and Rome about the same period as Tutankhamen, if one may judge from sculpture of the period. On p. 69 of the same work is a reproduction of two large female statues found in the tomb of the Egyptian nobleman, Mehenkwetre, who lived 4,000 years ago, each statue bearing a basket on the head supported by the left hand. The custom was not confined to one sex only. On p. 172, Anubis, the jackal-headed warden over the rites of embalming, is represented holding a weapon of sorts in the left hand. On p. 255 a wall sculpture is shown, representing the king offering incense to the God Sekhmet with the left hand! That the custom of using the left hand in making offerings was in use in these ancient times is amply proved or the artist would not have dared represent a king making an offering to a god in this way. These examples suffice to indicate that the left hand was freely used by the ancient Egyptians where the modern European would ordinarily use the right.

That the representation of the use of the left hand is not merely accidental on the part of the sculptor or artist is also indicated by the frequency of occurrence. Professor Wood Jones, in his book, "Principles of Anatomy as seen in the Hand," has shown that the modern artist is apt to be careless in detail. One could not argue from this that the ancient artists were equally careless. The fact that the whole of their script was the product of observation, not to mention the slower rate of living and the obvious patience with which the work was carried out, is sufficient to discount such an argument.

Among the uneducated coolie class of Chinese the use of the left hand is as common as the right. Among the more educated people, the majority use the right hand. If the parents had been taught to write, the use of the right hand in the children predominates, due, I believe, to the parents teaching the young child to use the right hand. Where, however, the parents are poor and uneducated and the children attend school, it is not uncommon for the child to write with the right hand and use the left for all other purposes. I have observed a large number of Asiatic infants and find that they extend the left hand just as freely as the right. In view of the fact that the right cortex of the brain can be so easily educated after damage to the left, it seems to indicate that there is no essential fundamental difference between the two sides. The more I study the problem, the more I am inclined to the opinion that right- or left-handedness is chiefly a matter of training in early life. The tendency to the use of one hand being established, the development of the various muscles, nerves and nerve centres goes on automatically.

If I may be permitted to digress somewhat from the main theme, I would point out that another interesting problem is raised by these facts, namely, the common origin of the Egyptians and Chinese from a single nucleus of people.\* I have indicated in my ethnological notes in connection with the Hylam crania

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\* *Biometrika*, 1928.

that there is some evidence to show that the Egyptians, Akkadians, Sumerians and Chinese have a common origin. There is a similarity in script, astronomy, religion and the hereafter, the making on earth of model palaces, worldly goods and servants to ensure the comfort of the departed in the next world. In the case of the Chinese the models are burned when the soul has passed through the phase corresponding to purgatory, and the smoke rises and becomes a spirit representation of the article burned. The Egyptians buried the models in the tomb with the body. If, perhaps, the evidence of common left-handed customs in Egyptians and Chinese is a rather weak link in the chain of evidence, it at least adds strength to the case, especially when one remembers that the near neighbours of the Egyptians, the Greeks and Romans, were apparently predominantly right-handed.

GORDON HARROWER.

### Africa, East: Religion.

Beresford-Stooke.

**An Akamba Ceremony used in times of Drought.** By G. Beresford-Stooke.

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Lindblom, in "The Akamba," Cap. XIV, Section 5, makes a brief reference to the connection of the medicine man with agriculture and rainmaking. The following is a more detailed account of the actual ceremony as practised to-day.

It not infrequently happens that the rains break and then suddenly cease, and the crops are jeopardised. When this happens it is customary to enlist the services of a medicine woman or man who specialises in work of this description. Rain-making ceremonies are usually carried out by women, presumably because the main work of raising crops devolves on the women, but there are men here and there who also specialise in this ceremony.

The medicine woman calls to her hut all the married women of the neighbourhood who have borne one or more children. An unmarried woman, or a married woman who has not yet given birth, is not eligible. Each woman must take with her a small supply of grain from the preceding harvest.

The ceremony begins at sunset when all the married women join the medicine woman in her hut. They beat drums and dance until about midnight, when they all go to sleep. During this time, no man or unmarried woman is allowed inside the hut, nor may any of those inside leave.

In the morning, the men and the young girls come and dance outside the hut. Meanwhile, inside, the medicine woman will tell two or three of the more influential women what the spirits have communicated to her in her dreams as to the prospects of rain and crops.

Three or four old men are then called to the door of the hut, and are told by the medicine woman to bring a black goat. When the goat arrives, they are told to take it to a certain tree in a certain sacred grove (*ithembo*). A single tree standing by itself may be an *ithembo*. A procession then takes place, headed by the old men with the goat, and consisting of almost all the inhabitants of the locality, including the married women, who now leave the hut. From this point onwards the medicine woman is no longer prominent. Her main function has been to commune with the spirits in a dream.

Each woman has to take with her a small portion of the grain she brought the day before.

On arrival at the *ithembo*, the goat is sacrificed by the old men in front of the tree, the rest of the people squatting round in a large semicircle. As the goat is cut, some of its blood is caught in a half-gourd. Meat from the ribs is taken, cut into small pieces and mixed with the blood, as also is a portion of the digested contents of the stomach.

Then two old men and two old women take up positions before the *ithembo*, the men on the right and the women on the left. The men hold between them the half-gourd of blood and slowly spill it at the foot of the tree, and the women drop grain. As they do this, they pray to *aimu* (the ancestral spirits) for rain, plenty of food, many children and deliverance from all evil.

At the foot of the tree is some sugar beer in a small gourd, placed there the day before. The old men pour a little of this on the ground, and the balance is consumed by the men present.

The intestines of the goat are then removed and cut into small pieces. Every person present receives a piece, and it is customary to take this home and put it in the water jar. Other pieces are taken and thrown all over the place on the roads and in the fields, as they are supposed to bring good luck.

The women then go home, and the men conclude the ceremony in the evening by a feast of beef and sugar beer.

This is a description of a typical ceremony, but it should be remembered that medicine men always have their own individual ways of doing things, with the result that details always vary.

G. BERESFORD-STOOKE.

### Obituary.

Keith.

**Sir William Selby Church, Bt., 1837-1928.** By Sir Arthur Keith, **106**  
F.R.S.

Sir William Selby Church, Bt., died on 27th April, 1928, at his residence, Woodside, Hatfield, where he was born on 4th December, 1837, being thus in his 91st year at the time of his death. He was a tall, heavily-built man, who retained his upright carriage and wide outlook on men and affairs until the very end. He left Harrow for University College, Oxford, in 1856, and took a first class in the Natural Science Tripos of 1860. That was the time when "The Origin of Species" was beginning to make history and when Huxley was proceeding to investigate the relationship of man to the animal kingdom. Under the influence of his teacher, Professor George Rolleston, William Church made a research on the "Myology of the Orang-utang," which was published in the first and second volumes of the *Natural History Review*, 1861, 1862—one of the earliest and most important of English contributions to the anatomy of the anthropoid apes. At the time he was making this investigation he became a student of medicine at St. Bartholomew's Hospital, London, but retained the Lees' Readership in Anatomy at Oxford until 1869. He became a Bachelor of Medicine in 1864, assistant physician to his hospital in 1867, and at the same time demonstrator of Zoology in its school. Soon after he devoted himself to medicine altogether, becoming full physician at St. Bartholomew's Hospital and in due time President of the Royal College of Physicians and subsequently President of the Royal Society of Medicine. Although he made no further contribution to the literature of Anthropology than the one mentioned, he never lost interest in this subject, becoming a fellow of the Anthropological Institute in 1874. He was a good physician and a great statesman in all kinds of public business. He took a keen interest in the welfare of the Institute and gave it the benefits of his wide experience in the management of its affairs.

ARTHUR KEITH.

### REVIEWS.

Europe: Archæology. Garrod and others. (Supplementary Publication of the Royal Anthropological Institute, 1928.) Plates I-VII, pp. 113. 25 text figs. Price 5s. net. **107**  
*Excavation of a Mousterian Rock Shelter at Devil's Tower, Gibraltar.*  
By Dorothy A. E. Garrod, L. H. Dudley Buxton, G. Elliot Smith, and Dorothea M. A. Bate. Appendices: By R. C. Spiller, M. A. C. Hinton and Paul Fischer. The Royal Anthropological Institute has done well to include as an "Occasional Paper" in its list of supplementary publications an account of the important

discoveries made by Miss Dorothy Garrod at Gibraltar during her excavations of 1925-26. It is a publication which will be of interest to all who wish to become familiar with the methods of modern archaeology and who desire to learn the details of one of the most important of recent discoveries of prehistoric man. Miss Garrod has associated with herself a galaxy of talent—Mr. L. H. Dudley Buxton to describe the human remains; Professor G. Elliot Smith to give an account of a remarkable brain; Miss Dorothea Bate to identify and describe the species of mammals and birds which her excavations brought to light in association with prehistoric man; Mr. M. A. C. Hinton to give his expert opinion on certain voles; Mr. Paul Fischer to identify and explain the various shells which were found; and Mr. R. C. Spiller to give a geological analysis of the materials excavated. Modern archaeology demands “team work,” and this publication is an excellent example of its efficacy.

With the full account before us we can now sum up the new facts which Miss Garrod's discovery adds to our knowledge of prehistoric man. We knew that men of the Neanderthal species had lived at Gibraltar; the very first discovery of the species was made there in 1848. But we did not know when they lived there. At the Devil's Tower rock shelter, at the foot of the northern face of the Rock, Miss Garrod has proved that they were in existence during the later part of the Mousterian period. In all six strata which filled up this rock shelter she found quartzite and bone worked in the later Mousterian manner. The massed accumulations had a depth of 28 feet; it was in the fourth stratum, counting from above, that the fragments of a skull of a Neanderthal boy about five years of age were encountered. The Mousterian deposits of the rock shelter were based on the Mediterranean basin; it was subsequent to the deposition of this beach that Neanderthal man took up his abode in the Devil's Tower Rock Shelter. This is another of the important facts which we owe to Miss Garrod, for it brings the Mousterian strata of the Grotte du Prince, Mentone—also situated on the 50-foot beach—into line with those she had excavated at Gibraltar. It also brings the deposits in Ghardalam, Malta, into the same series (*Journ. Roy. Anthropol. Instit.*, 1924, vol. 54, p. 274)—a fact which has a direct bearing on a matter discussed by Miss Garrod: whether Gibraltar and Africa were connected by a land bridge in Mousterian times. Miss Garrod—rightly, in my opinion—denies the existence of such a

bridge at any time in the latter half of the Pleistocene period, and is prepared to accept the opinion of French geologists that Europe and Africa have been completely separated since the latter part of the Pliocene. An observation by Mr. Hinton seems to me to bear on this point. He compares the Pleistocene vole (*Microtus brecciensis*) found by Miss Garrod at Gibraltar with another form which occurs in Pleistocene deposits of Corsica and Sardinia, and comes to the conclusion that they must be regarded as generically different. To produce such a difference, it seems to me that we must postulate a very long period of isolation. Still, the fact remains that Neanderthal man, of the later type, did reach Malta, and on the evidence as it now stands we must infer he reached it by crossing a considerable stretch of sea. Further, Mr. Fischer finds that the mollusca of the 50-foot beach at the Devil's Tower differ markedly from those which occur in the deposits of the rock shelter. A considerable period seems to have elapsed between the deposition of the 50-foot beach and the coming of Neanderthal man to Gibraltar, Mentone and Malta.

The human skull, although fragmentary, throws a welcome light on the childhood of Neanderthal man, for until now we have had only that discovered at La Quina—the skull of a girl about eight years of age; whereas the Devil's Tower skull (Gibraltar II) is that of a boy about five years of age. The age was undetermined until the jaws were found in the second season of digging. I saw the fronto-parietal parts at the end of the first season and, guided by what we know of the age changes in modern man, I adjudged the boy to have been about 12 or 14 years of age; the discovery of the state of the dentition gave the true age and showed that certain cranial changes occurred much earlier in Neanderthal man than they do in us. New facts, too, are the large pulp-cavities of the milk molars and the very striking specific characters of the temporal bone—which Mr. Dudley Buxton describes. Most remarkable of all was the cranial cavity, indicative of a large brain, but, as Professor Elliot Smith insists—even allowing for growth after the fifth year—the Gibraltar boy's brain would have fallen short of that of the La Chapelle-aux-Saints man. It is true that the frontal lobe is massively developed, but it is certain that its massiveness and steepness would have become less apparent as the vertical forehead of youth took on the slope of maturity. Remarkable, too, is the width of the brain cavity: it was certainly 140 mm. wide; its length may be estimated



at 162 mm.; the width is 86 per cent. of the length; it is a "brachy" brain. In Gibraltar I—the skull of a woman—the corresponding dimensions are 166 mm. × 34 mm., the width being 80 per cent. of the length. Both Gibraltar I. and Gibraltar II have wide foreheads; the minimal width in both is 102 mm.; in La Chapelle it is 109 mm.: all have wide foreheads. The two Gibraltar skulls belong to the same breed and have also close resemblances in general form to the La Chapelle skull. All three have the flattened bun-like brain of the great anthropoid apes. In the La Quina skulls we have another type—elongated, with sides more or less vertical and appressed. Now, this is of great interest because of the discovery at Taungs; the young anthropoid discovered there had an elongated head with appressed sides. Lately it has been discovered that the appressed elongated type occurs as a variation among gorillas. This difference in cranial type does not seem to indicate much more than a local or family difference; it is not a character which has even a racial value.

Certain discoveries are revolutionary in their effect; we cannot fit them into our preconceived picture of man's prehistory; to fit them in we have to take our preconception to bits and build the whole up again into a new picture. Miss Garrod's discovery is not of this revolutionary nature; it amplifies our picture of South Europe and the Mediterranean area in Pleistocene times; it fills in details concerning this strange race we call Neanderthal; but it is not necessary to alter the outlines of the theory which guides most of us as we go forward.

The monograph now issued reflects great credit on the authors concerned and on British archaeology. And yet there are one or two points over which we may cavil. Mr. Dudley Buxton apologises for not giving accurate drawings of this important specimen. It is true photographs are given and many outlines, but these are poor substitutes for faithful drawings—such as engineers give—of an important specimen such as this skull certainly is. Accurate drawings do not take many hours to make and save paragraphs of dry description. The authors have adopted a strange plane—one passing through nasion and lambda. The plane adopted, however, is a minor matter if only drawings of the various views are made to scale and all details filled in. ARTHUR KEITH.

Roumania: Folklore.

*Paganism in Roumanian Folklore.* By Marcu Beza. Dent & Sons, 1928. Pp. x + 162; 19 plates and 4 illustrations in text. Price 7s. 6d.

Beza.

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This pretty little book—for it is pleasant to handle, well printed, and furnished with attractive pictures—may well serve as an introduction to the rich folklore of Roumania. To those who have made any study of the subject, however, it offers little or nothing that is new. The ten chapters, which were originally lectures, contain for the most part legends of well-known types, e.g., Chapter I gives some fairly familiar tales concerning the Virgin Mary, VI and VII stories of the *Fairy Bride* kind, VIII creation-stories, including an amusing account of how the Devil stole Adam's rib and lost his tail to a pursuing angel, with the result that God, who was sleepy at the time, created woman out of that unpromising material. Several chapters, however, treat also of popular customs (Christmas mummings in Chapter I, moon-magic in Chapter II, the *paparude* and *kalojan* in Chapter III, funeral and midsummer ritual in IV and V respectively). Chapter X draws some fairly obvious comparisons between Roumanian and Scots ballads; the subject is one which is by no means exhausted, but hardly capable of fruitful treatment without going into a much wider topic, European folk-poetry in general.

The author has had every opportunity for becoming acquainted at first hand with the beliefs and customs of his native country. In England he has acquired, not only a very serviceable knowledge of our tongue, but also an acquaintance with the works of Frazer and other excellent authorities. It is, therefore, to be hoped that he will give us more and more detailed accounts of what he knows from observation and reading; for Roumania, and indeed the Balkan countries generally, are but poorly represented in English works on folklore. He would be well advised in future works to save space by omitting most of the superficial parallels which he is fond of drawing, especially with classical traditions; the reviewer has found them singularly unconvincing, for the most part. The fact seems to be that the pagan survivals in that part of the world, apart from a few obvious traces of the Brumalia in the Christmas customs, and perhaps the connection alleged to exist between Rosalia and Rusalii, are inherited from Slavonic paganism, and not from either Rome or Greece.

H. J. ROSE.

Malayo-Polynesia: Ethnology. Schroder.

*Über die semitischen und nicht indischen Grundlagen der malaiisch-polynesischen Kultur.* Buch I. *Der Ursprung des ältesten Elementen der austro-*

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*nesischen Alphabete.* Von E. E. W. Gs. Schröder. Pp. 88. Medan: Köhler & Co., 1927.

This is the first section of a work designed to prove that Malayo-Polynesian (*alias* Austronesian) culture was originally based rather on Semitic than on Indian influences, and it deals chiefly with the alphabets of the Malay Archipelago, especially Sumatra. The Javanese alphabet is too obviously Indian, of course. But the author claims that all the rest are derived direct from Semitic scripts of the Mesopotamian region (or thereabouts) of the 7th century B.C., and not from or through a southern

form of the Indian Brahmi alphabet, as is now the generally accepted view. The case is argued point by point and illustrated by a number of very clear palæographical tables. It turns mainly on resemblances in detail of certain letters, which it would take too long to discuss here.

The whole thing is very ingenious, but I confess it leaves me unconvinced; and the supporting arguments drawn by the author from the supposed Semitic origin of certain Austronesian words in no way strengthen his contention. In my opinion these etymologies are, frankly, impossible.

C. O. BLAGDEN.

## CORRESPONDENCE.

Ireland: Archæology.

### Sligo Temperature.

To the Editor of MAN.

SIR,—Without entering on the 110  
Sligo controversy, I would ask that isotherms should be referred to, rather than latitudes. At present the winter line of 42° passes the Hebrides, Sligo, Waterford, Anglesey, South Devon, and Bordeaux, thanks to the Gulf Stream. The history of the Gulf Stream in the past is the key to the question of temperature.

Yours faithfully,

FLINDERS PETRIE.

Petrie.

have existed in North-West Ireland. Even a casual reading of the relevant paragraph in our memoir would make it quite clear that it is the discovery of the remains of extinct animals in Ireland, such as are found usually with palæolithic remains elsewhere, which constitutes the chief evidence upon which we rest our *a priori* belief in this matter. (See p. 9, Sub-Sec. b). Prof. Charlesworth, however, does not mention this fact, and, on his own showing, is criticising our views "from memory," which, in scientific matters, is fortunately not a usual practice. Professor Charlesworth asserts that any human artifacts discovered between or in the drifts of East Anglia demand, of necessity, an ice oscillation only. Though, no doubt, he possesses intimate and peculiar knowledge of East Anglian conditions, this claim is, nevertheless, unsupported by any evidence, and, as such, may be disregarded. Again, it is claimed "on the clearest geological grounds" that the shelter at Rosses Point cannot be of Early Mousterian age. If, by this, Professor Charlesworth wishes to infer there is no further geological discussion possible upon this matter, he is once more entirely out of his reckoning. This fact will, however, no doubt be made more clear to him in the coming autumn, when we propose to put forward our complete paper upon the Sligo discoveries. In the meantime, we wish to state, even at the risk of offending what we can but regard as Professor Charlesworth's parochial views on man and the Ice Age, that there have been found by one of us (J. P. T. Burchell), *in situ*, at the base of the lowermost Boulder Clay on Coney Island and at Ballyconnell in Sligo, and by Mr. C. Blake Whelan in an inter-glacial deposit at the latter locality, certain specimens which by their forms and

Ireland: Archæology. Moir: Burchell.  
Ireland in Pleistocene Times.

To the Editor of MAN.

SIR,—We notice that in the 111  
current number of MAN (84, July, 1928) there appears a note by Professor Charlesworth which purports to be an answer to that of Mr. C. Blake Whelan in the same publication (54, May, 1928). As we entertain no doubt that Mr. Whelan is quite capable of dealing with the various statements and claims regarding his opinions, which Professor Charlesworth puts forward, we do not propose to deal with these. But it is otherwise with certain assertions regarding our own publications and views upon the Sligo artifacts and the geological evidence we have marshalled in regard to them, and it is upon these assertions that we wish to comment briefly.

It is entirely misleading to claim that in our recently published monograph, "The Early Mousterian Implements of Sligo," we argue that because of the "astronomical positions" of Sligo, East Anglia and Spain, we believe that Palæolithic Man should

flaking can only be referred to the activities of Lower Palæolithic Man. Further, such discoveries, outrageous and disruptive of the old order of things as they may appear to Professor Charlesworth, cause no surprise to those who are familiar with recent advances in our knowledge of such matters.

Finally, we would comment on Professor Charlesworth's appeal to archaeologists who have accepted the human origin of the Sligo specimens that they should visit the actual sites where the discoveries have been made. Though, no doubt, this would be a pleasant excursion and probably convince them of the truth of our geological conclusions, we, nevertheless, do not consider it to be necessary, any more than in deciding upon the human origin of, for instance, a wheelbarrow, it would be needful to pay a visit to the workshop where it was made.

J. REID MOIR,

J. P. T. BURCHELL.

6th July, 1928.

#### Ethnology.

Perry.

#### "The Children of the Sun."

To the Editor of MAN.

112

SIR,—I cannot allow one statement in Sir J. H. P. Murray's letter to you (MAN, 1928, 100) to pass unnoticed. Speaking of Chinnery's article (*J.R.A.I.*, 49, 1919, 271 *seq.*), he says that "his article had nothing to do with pearls, and therefore, naturally, he did not mark them on his map either at the Trobriands or anywhere else. What puzzled me in Mr. Perry's map was, not that it does not show pearls, but that it shows them in the wrong places."

I got my distribution of pearl beds from Chinnery's paper, and, as I have already said (MAN, 1926, 151), did not indicate pearl-shell in the Trobriands for the simple reason that I did not know of their existence in that archipelago. If Sir Hubert will look at the second edition of "The Children of the Sun," he will see that I have indicated pearl-shell in the Trobriands, in accordance with his criticism. I have also rectified the distribution of gold in the upper waters of the Fly River. Chinnery remarks in his paper: "Pearls certainly occur on the coast of New Guinea in every locality which has yielded traces of the immigrants, and I have already pointed out in which of them gold occurs" (*J.R.A.I.*, 49, p. 284).

I do not propose to open up the subject in detail once again, but I cannot refrain from adding another quotation from Chinnery: "What induced the immigrants

"to penetrate the interior is, I think, strongly suggested by the pestles and mortars and their discovery in gold-fields. Many of them are eminently suited for the purpose of crushing stone, and I feel strongly inclined to support the view expressed by the practical miners of the Yodda Goldfield" (p. 285. 287).

I think that Sir Hubert Murray will agree with me, when he sees Chinnery's article, that our purposes were far from different. Chinnery was simply testing a theory that I had previously propounded, in "The Megalithic Culture of Indonesia" (Chap. 22) and elsewhere, that peoples with the culture that I termed the Archaic Civilisation, in "The Children of the Sun," were attracted to certain places by their stores of gold, pearl-shell, and so forth. Perry states that there is a definite relationship between the distribution of megalithic monuments and ancient mines and pearling beds in other parts of the world; the regions from Indonesia to Easter Island are especially included in this survey of pearling beds. . . . But it is when we come to examine Perry's work on Indonesia that the relationship of the New Guinea cultures to Indonesia is most strongly suggested" (*loc. cit.* 284).

I may perhaps be allowed to take this opportunity of stating that the work of Dr. A. P. Elkin on the mythology and ritual of Australia has afforded strong confirmation for this thesis. He has shown that Australian culture arrived in two streams, proceeding from the neighbourhood of Cape York and the north-west respectively. These streams are easily distinguishable. They proceed in both cases from the close neighbourhood of pearl-beds. The use of pearl-shell strongly confirms this conclusion. For they are sought from these districts and taken many hundreds of miles into the interior. Dr. Elkin is now in the north-west, engaged in research under the auspices of the Department of Anthropology in the University of Sydney. He tells me that the curious markings that are made on the pearl-shell pendants worn by the Australians are made by men of the Broome district, whence comes the most prized pearl shell.

The evidence coming from the Solomons, and other parts of Southern Melanesia, suggests that the pearl-shell centres are those where the influence of the archaic civilisation is most active. The work of Dr. Ivens in Mala is strongly suggestive of this correlation.

W. J. PERRY.



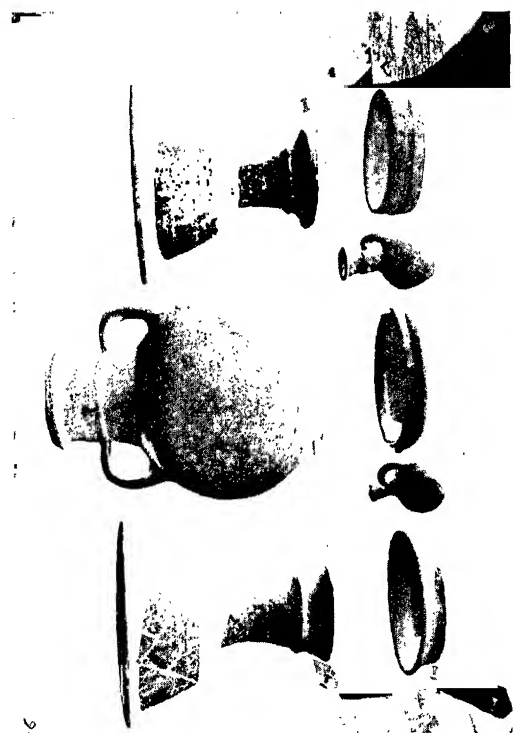


FIG. 2.



FIG. 4.



FIG. 1.



FIG. 3.

EXCAVATION IN SOUTH PALESTINE.

## ORIGINAL ARTICLES.

With Plate I-J.

Palestine: Archæology.

Petrie.

**Excavation in South Palestine.** By Sir Flinders Petrie, F.R.S., **113**  
*F.B.A. With Plate I-J.*

The desert south of Palestine was scarcely known before the survey by Lawrence and Woolley in 1911-14. As this was the region most open to Egyptian influence, it has the best connection with known history, and deserves full attention. The past two winters' work in this district have produced much more detail of the archæology of Palestine than was known before. The ruins of the successive cities of Gerar (now Tell Jemmeh) were searched through a depth of thirty feet; six times had the place been rebuilt by the Egyptians or others, at levels averaging five feet apart, and the known history of the invasions gives a precise dating to each of these cities. Thus the British School of Archæology of Egypt has carried forward the details known in Egypt into the field of Palestine archæology. Seven hundred forms of pottery have been dated, and many types of beads, and all published in the volume "Gerar." Further, on this basis the city and cemeteries of Beth-pheet (now Fara) have been opened up, and a long series of perfect specimens recovered, dating from 1500 to 700 B.C.



FIG. 1.—GERAR. SICKLE FLINTS, WITH PLASTER SETTING.

The whole of the country south of Gaza is evidently much drier than it was anciently, and of the sites of 38 cities named in that region in the Book of Joshua only two are still occupied sites—Beersheba and Ziklag. Half of the names can still be found in their Arabic forms on the present map, though only applied in most cases to a small district, the actual settlements having disappeared, many in the sand dunes.

The historical connections can be traced in the remains. The smelting of iron on a large scale in 1150 B.C. is shown by three furnaces, by the slag left behind, and by heavy agricultural tools, from which we gather that iron was as common there at that date as it is now. This accords with many references to iron at the time of the Israelite invasion. Gold also appears mainly at the time of the great abundance of it among the Midianites recorded in the Book of Judges. The wealth of Palestine was greatest in the age of Solomon, the fine jewellery being much better than in Egypt or Babylonia at that time. The hold on the trade routes between East and West was the cause of this prosperity. The Red Sea route to Persia, India, and Africa was drawn to Ezion-geber in place of Suez, and the northern trade was tapped at the Euphrates before reaching the Mediterranean. During the weakness of his neighbours Solomon was the international middleman.

In Gerar various evidences were found of a movement from middle Asia about 970 B.C., bringing objects known in Siberia, and the model chariots with smooth and spiked wheels as found at Anau and as far east as the Oxus. This movement seems likely to have been headed by Sheshenk I, whose name is "the man of Susa," on his way to the acquisition of Egypt. Jars of cremation burials of this age are likely to belong to this movement. With this may be linked the movement of the Etruscans from the same region, as new evidence has appeared

of their familiarity with Babylonia and the Persians, described fully in *Ancient Egypt* for September. The Etruscans seem to have come over into Lydia between 1000 and 900 B.C., and then passed on to Italy about 850 B.C. The whole migration would be closely parallel to the Turkish troops being employed in Egypt by 1020 A.D., and increasing in power till they held the country in the next century, having also occupied Asia Minor in 1080. It seems possible now to have a connected view of the changes in the tenth century B.C., when the Eastern movement reached the Mediterranean along two lines, Egypt and Anatolia. FLINDERS PETRIE.

## DESCRIPTION OF PLATE I-J.

- Fig. 1.—Beth-Phelet. Bronze knives, Ramessid, 1300 B.C.  
 Fig. 2.—Beth-Phelet. Pottery of the Solomon period.  
 Fig. 3.—Beth-Phelet. Necklace of sard and gold earrings, Solomon period.  
 Fig. 4.—Beth-Phelet. Silver ladle for wine, about 800 B.C.

## Britain: Archæology.

Dunning.

**Second Interim Report on the Excavation of a Bronze Age Tumulus at Dunstable, Bedfordshire.** **114**  
*By G. C. Dunning.*

During July, 1927, the University College (London) and Hospital Anthropological Society continued the excavation of No. 5 Barrow, Five Knolls, on the Dunstable Downs. Trenches, 4 feet wide, were cut on both sides of the trench cut in 1926, so that the whole of the central area and a large part of the periphery of the mound have been examined down to the undisturbed chalk (*see Plan, Fig. 1*). Twelve secondary interments by inhumation were found, including a large multiple burial near the N.E. margin of the barrow, and it is now possible to suggest a more definite period for the total of twenty-two superficial burials by inhumation.\*

*Skeleton No. 11.*—Scattered fragments of an adult male, 12 inches below surface. Small skull fragments, vertebræ, shaft of radius finger bones, parts of both innominate bones, and head of femur.

*Skeleton No. 12.*—Pelvis and legs of a boy aged about 16 years, 6 inches below surface. The upper part of the right femur was displaced and lying by the side of the pelvis. Stature, 5 feet  $1\frac{3}{4}$  inches; platymeric index, 79·6; platynemic index, 67·2; "squatting facet" on tibia. Part of the shaft of an ox's tibia lay near the upper end of the boy's left femur, but the apparent association may be fortuitous.

*Pottery Fragments.*—Four small pieces of black pottery were found close together 12 to 18 inches below the surface, about 4 feet W. of the centre of the barrow. The paste is rather hard with sparing flint grit. The decoration consists of rows of horse-shoe shaped cord-impressions above and below the shoulder, formed by pressing a loop of cord into the clay with the finger-tip (*Fig. 3, No. 1*). Mr. Reginald A. Smith considers these fragments to be pagan Saxon, and they appear to represent a cinerary urn which has been disturbed and removed.

*Skeleton No. 13.*—Disturbed skeleton lying extended, chest downwards, arms along the sides and forearms crossed behind lower lumbar region, legs extended and feet close together. The lower jaw was in position but the skull missing; the pelvis was displaced and lay across the shafts of the thigh bones, and the right ulna lay across the pelvis. Very muscular male aged about 25 years; stature, 5 feet  $9\frac{1}{2}$  inches. Lower jaw robust with high ramus, teeth regular and incisors ground flat. The first molars are ground flat; the third molars are scarcely worn, rotated, and project considerably above the level of the other molars.

Platymeric index, 80·0; platynemic index, 70·8. The left tibia is fractured in its upper third, the bone being shortened by  $\frac{1}{2}$  inch, the lower fragment is displaced forwards, and the callus formation very marked. There is an extensive

\* MAN, 1927, 12.

calcification in the anterior common longitudinal ligament on the right side of the first and second lumbar vertebræ, perhaps due to the organisation of a blood-clot on the origin of the psoas muscle, as there is little osteo-arthritis in the other vertebræ.

*Skeleton No. 14.*—Found just below turf. Skull, lower jaw and right humerus missing. Lay on back, turned slightly to left side; left arm extended across the

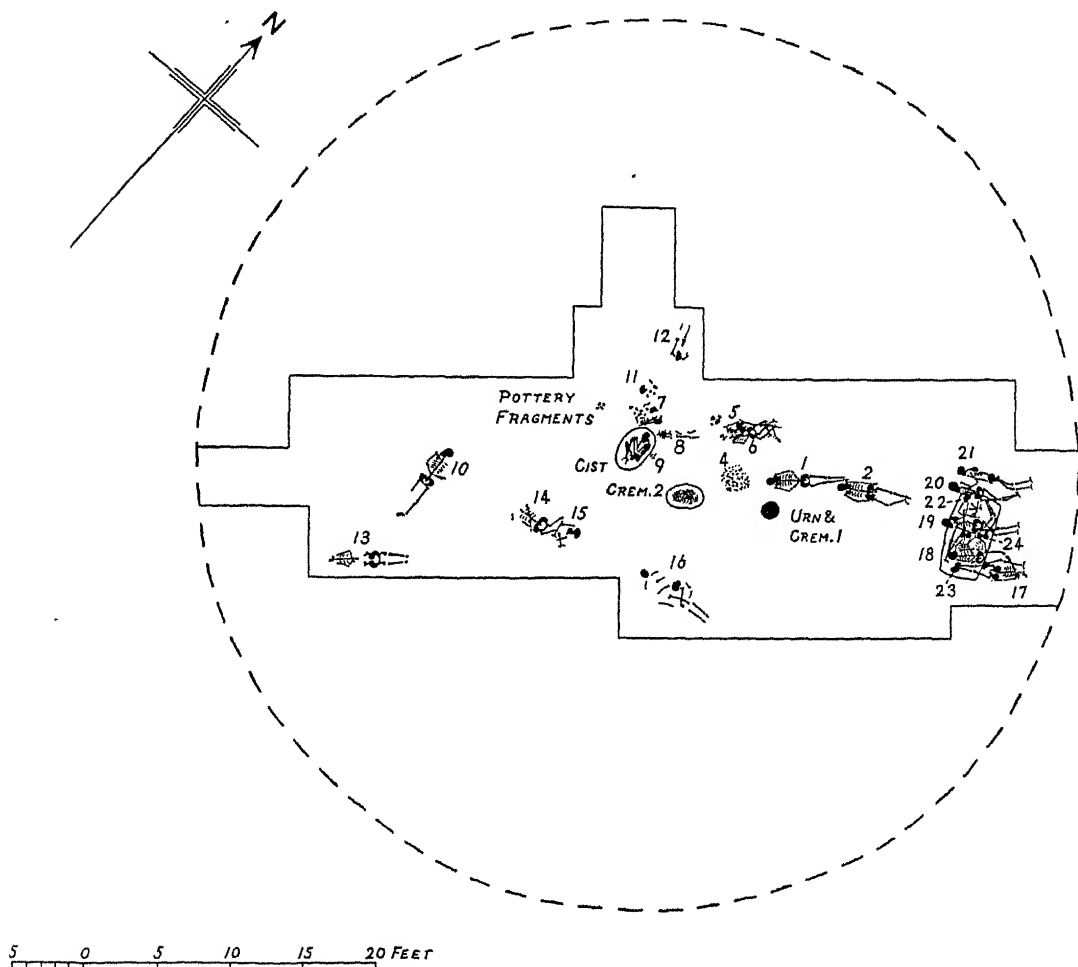


FIG. 1.—PLAN OF BARROW NO. 5, FIVE KNOLLS, DUNSTABLE DOWNS.

body behind the back, right arm down side, with the hands together near the right ilium. Thighs and knees flexed.

Well-built adult male, with rather slender arm-bones; stature, 5 feet 5 $\frac{3}{4}$  inches. Platymeric index, 82.7. Tibia bowed forwards, much gouged; platynemic index, 72.2.

*Skeleton No. 15.*—Much disturbed fragments, partly under the legs of No. 14, comprising lower jaw, few vertebræ, left humerus and both forearms, part of pelvis, and lower part of right femur. Adult male, stature (calculated from humerus) about 5 feet 3 inches. Lower jaw of slight build with short square ramus, molars ground down exposing pulp cavities, and right second molar lost before death.

*Skeleton No. 16.*—Disturbed skeleton lying with vault of skull only 3 inches below surface, extended on back with shins and feet in position. Head raised above



level of body and facing feet. The back-bone had been removed, arms and pelvis displaced and broken, and the thigh bones removed and then replaced in a crossed position. Powerfully-built male, aged about 28 years; stature, 5 feet 7 inches. Skull large and eyebrow ridges moderately prominent, palate regular with molars moderately worn, third molars reduced. All sutures open except sagittal, in which closure had commenced in the pars obelica. Cephalic index, 75·6; nasal index, 53·7. Limb bones heavy and muscular; platymetric index, 70·8; platycnemic index, 80·8; slight "squatting facet" on tibia.

*Multiple Burial.*—Near the E. margin of the barrow was a large burial of eight skeletons (see plan of barrow, Fig. 1 and Fig. 2), consisting of a rectangular grave with the long axis N.N.E.-S.S.W., 6 feet long, 3 feet wide, cut 15 inches in a hard chalk with vertical sides and flat bottom. This grave contained three

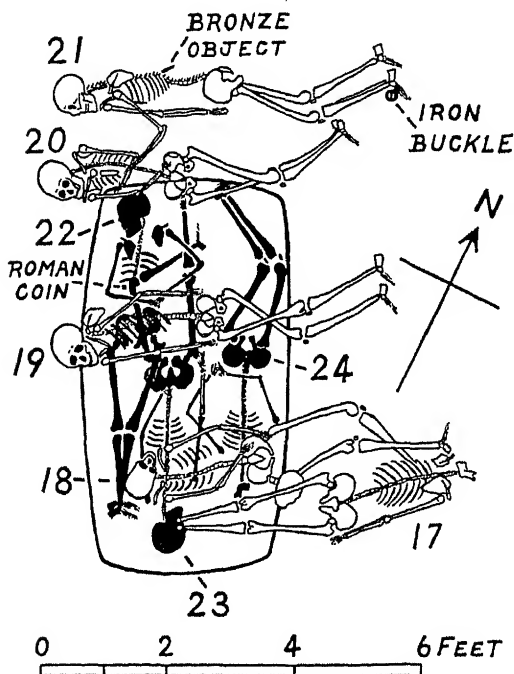


FIG. 2.—PLAN OF MULTIPLE BURIAL NEAR EDGE OF BARROW NO. 5.

skeletons (Nos. 22, 23, 24), and five more skeletons (Nos. 17–21) lay in the earth immediately above the grave, two across each end and one at the middle.

In the earth above the grave, and scattered between skeletons Nos. 17 and 18, were some small pieces of Roman pottery: (1) Samian ware, (2) bevelled rim, perhaps a lid, hard gritty light red surface, grey core; and between Nos. 18 and 19 was a bent iron nail, length 2 inches.

*Skeleton No. 17.*—Lower jaw in position, but skull and left arm missing. Lying extended chest downwards with right arm down side, legs extended and feet close together. Immediately under the shaft of the left femur, and above the right femur of skeleton No. 18, lay a detached skull on its right side facing south. This skull does not belong to No. 17, but to No. 24, a skeleton in the grave.

Young adult male; stature, 5 feet 3½ inches. Lower jaw robust with wide

square ramus; teeth regular, incisors ground flat, and third molars reduced; the right first molar is carious. Limb-bones short and stocky; platymetric index, 70·3; platycnemic index, 60·8.

*Skeleton No. 18.*—Extended on back, turned slightly to left side, with head raised above body and turned to left. Arms down sides and hands on left ilium, legs crossed. Young adult male, aged about 25 years; stature, 5 feet 7¼ inches. Skull robust, with very wide face. Teeth regular, overlapping bite, upper third molars reduced, lower third molars unerupted. Ramus of lower jaw low and symphysis deep, right second molar carious. Cephalic index, 77·8; nasal index, 50·2. Limb-bones muscular; platymetric index, 75·0; platycnemic index, 74·3.

*Skeleton No. 19.*—Extended on right side with head raised and turned to right. Arms along the sides and hands at sides of pelvis, thighs crossed and feet together. Young male, aged 20 years; stature, 5 feet 8 inches. Skull markedly asymmetrical with wide, flat face. Palate regular, overlapping bite, third molars reduced, and neck of upper right third molar carious.

Cephalic index uncertain owing to fracture at glabella, estimated at 83.0. Platymetric index, 77.7; platycnemic index, 73.8; slight "squatting facet" on tibia.

There is a healed fracture of the shaft of a left middle rib.

*Skeleton No. 20.*—Lying extended on back, turned to right, with head raised and turned to right; right arm down side and hand over pelvis; left arm down side, elbow flexed, and hand over right side of thorax; legs extended, and feet close together. Young male, aged 20–25 years; stature, 5 feet 7½ inches.

Skull small, with pronounced eyebrow ridges, short and wide face. Palate regular, first and second molars worn down, third molars scarcely worn or reduced. Ramus of lower jaw short, wide, and vertical. Cephalic index, 80.1; nasal index, 54.8. Limb-bones slender; platymetric index, 69.5; platycnemic index, 63.4.

The centra of the first and second lumbar vertebræ are ankylosed.

*Skeleton No. 21.*—Extended lying on right side, with head turned to right; right arm down front of body and hand near pelvis; left arm away from body, elbow flexed and hand over left forearm of No. 20; legs close together. Young male, aged 20–25 years; stature, 5 feet 8 inches. Skull rugged, with heavy eyebrow ridges. Palate regular, overlapping bite, third molars reduced, upper right canine carious and both lower first molars lost before death and alveolus absorbed. Cephalic index, 79.7; nasal index, 47.2. Limb-bones robust; platymetric index, 65.0; platycnemic index, 70.1; "squatting facet" on tibia.

Behind the lower ribs was a bronze object (Fig. 2 and Fig. 3, No. 2). It consists of a thin sheet of bronze bent into a tapering tube, the free end closed, the other covered by a leather sheath which is pinched together, as though by a suspending cord; total length, 3.15 inches. It appears to be of Roman or Saxon date.

Near the right foot was an imperfect iron buckle (Fig. 2, and Fig. 3, No. 3); length along tongue, 1.8 inches. This buckle may also be of Roman or Saxon date.

*Skeleton No. 22.*—Lay extended, chest downwards, with head turned to left, arms away from body, elbows flexed, and hands together near right ilium; feet above one another. Very muscular female, aged about 30 years; stature, 5 feet 2½ inches. Skull unusually muscular and eyebrow ridges prominent for a woman; coronal and sagittal sutures almost closed inside, union of sagittal externally at pars obelica; nose narrow and high bridged; palate regular, with edge to edge bite; teeth much worn exposing pulp, upper first molars lost before death; chin prominent. Cephalic index, 81.0. Limb bones and pelvis very muscular; platymetric index 74.2; platycnemic index, 73.3. The lower ends of both ulnæ are fractured.

*Skeleton No. 23.*—Extended on back with head turned to right; right arm down side and hand near upper end of right femur; left arm down side and hand under sacrum; right leg fully extended; left leg flexed at knee, and lower end of tibia passing under upper end of right tibia. Slenderly built male, aged about 25 years; stature, 5 feet 6¼ inches. Sagittal suture closed at pars obelica, palate narrow, overlapping bite, lower incisors slightly crowded, upper third molars

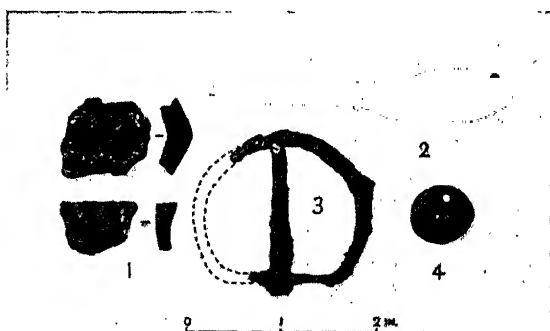


FIG. 3.—1. SAXON POTTERY. 2, 3, 4. OBJECTS ASSOCIATED WITH MULTIPLE BURIALS.

reduced. Cephalic index, 77·6. Limb bones long and slender; femur much bowed; platymeric index, 73·7; platynemic index, 63·5.

Near the lower end of the left femur was a worn, pierced coin of Claudius Gothicus (268-270 A.D.) (Fig. 2, and Fig. 3, No. 4). This is the only coin found during two seasons' excavations, though Roman remains are plentiful in the Dunstable district. It seems more likely that the coin fell or was placed in the open grave than that it worked through the soil to the bottom of the grave, underneath skeleton No. 22. The Saxon custom is well known of wearing pierced Roman coins on the necklace as pendants.

*Skeleton No. 24.*—Extended chest downwards, with lower jaw turned to left; skull detached and lying between femora of skeletons No. 17 and 18; arms down sides, elbows flexed, and hands together near right ilium; legs slightly flexed to left, with knees pressed against side of grave and feet close together against end of grave. Slenderly built male, aged 20 years; stature, 5 feet 5 inches. Skull asymmetrical, palate regular, overlapping bite, lower right first molar carious. Cephalic index, 78·0.

Right humerus shows a supratrochlear foramen. Platymeric index, 85·1; platynemic index, 67·5; "squatting facet" on tibia.

*Dating of the Superficial Burials.*—The archaeological evidence for dating these burials is very scanty. Accepting the evidence of the coin found in the grave, the end of the third century A.D. may be taken as the lower limiting date, but there is nothing to indicate how much later the burials may be. Indirect evidence is found in the crossing of the hands behind the back, noticed in five adult male skeletons (Nos. 1, 10, 13, 14, and 24) and one adult female (No. 22), and the careless method of burial, in some instances the body having been apparently thrown down into a shallow pit dug in the top of the barrow, suggesting that the barrow was perhaps used as an execution ground. Many of the skeletons have been disturbed subsequent to burial, probably quite recently.

#### *Physical Characters of the Skeletons.*

*Skulls.*—All the adult male skulls are large and muscular. One skull is dolichocephalic, six are mesaticephalic, and four brachycephalic; the cephalic index varies from 74·7 to 83·0 (?); the average index of the six male skulls from the multiple burial is 79·4, the average of the other male skulls is 78·6, so that the whole of the superficial burials may provisionally be considered together.

The average cephalic index of 52 male Anglo-Saxon skulls in London Museums is 74·7, definitely lower than the average for the Dunstable skulls, and probably to be explained by Alpine admixture in the latter. The face is larger and much broader than in Saxon skulls, and the nasal index higher (average of 7 males, 50·6). On the other hand, close agreement is found with the characteristic greater basio-bregmatic height and nasio-basion length of the Anglo-Saxon skull, distinguishing it from the Iron Age and Romano-British types.\*

The teeth are large and the palates regular; in only two lower jaws is there crowding of the incisors, and two at least show an edge-to-edge bite. Reduction of the third molars is marked, and seven adult males show caries, most frequently in the first molar.

*Age.*—All males are adult, but under 30 years; age most between 20 and 25, and one below 20 years. The infant (No. 9) may be Bronze Age.

*Stature.*—The stature of 14 male adults, estimated from the right femur by using Professor Pearson's formula, varies from 5 feet 3½ inches to 5 feet 9½ inches; average, 5 feet 6½ inches. The average stature of two female adults is 5 feet 3½ inches.

\* Morant, G. M. (1926), "A First Study of the Craniology of England and Scotland, from Neolithic to Early Historic Times," *Biometrika*, 18, p. 85.

*Radio-Humeral Index.*—Varies from 64·3 to 76·0; average for 13 male adults is 70·5, brachycercic.

*Tibio-Femoral Index.*—Varies from 73·2 to 83·6; average for 14 male adults is 78·2, brachynemic.

*Platymeric Index.*—Varies from 64·6 to 85·1; average for 15 male adults is 74·3, platymeric.

*Platynemic Index.*—Varies from 60·8 to 80·8; average for 15 male adults is 68·6, mesocnemic.

*"Squatting Facet."*—Present in eight male adults (53 per cent.) and one female adult.

*Fractures of Forearm.*—It is notable that the lower ends of the ulnæ of the two female skeletons show healed fractures. This has been noticed in Predynastic Egyptian skeletons, and explained by Professor Elliot Smith as the result of fending blows from sticks.

#### Summary.

1. Barrow No. 5 was erected in the Early Bronze Age. The primary burial contained a woman of Neolithic type.

2. The urn containing cremation No. 1 was deposited in the Middle Bronze Age. Although there is no actual evidence, it is very probable that cremation No. 2 dates from this period.

3. The barrow was perhaps used as a burial place in Saxon times.

G. C. DUNNING.

#### India: Religion.

Ayyar.

**Chathan: a Devil or Disease?** By K. V. Krishna Ayyar, M.A.

115

In September and October last there occurred in a family living in Calicut (India) certain extraordinary incidents which the common folk regard as the work of Chathan or the imp of mischief in South Indian demonology. One morning some human excreta were noticed on the outer side of the kitchen door leading to the backyard of a certain house. This did not arouse any suspicion, as it was thought to be the trail left by some bandicoot in its nocturnal rounds through gutters and latrines. Next morning the same thing was noticed in the same place. The womenfolk were alarmed, but the father of the family pooh-poohed their fears and went to his work as usual. But the third day the filth was found within the kitchen, in the hearth itself. The hand of Kutti-chathan was unmistakable, for is not night soil the most favourite weapon in his armoury? From that day, for the space of nearly two months, the family was subjected to the most harassing torment that human mind could conceive of. There was a horrible uncertainty hanging about the whole house; there was no peace, no sleep; they knew not whether and when they could enjoy a meal. The lot of Tantalus seemed to have fallen to them. They dared not open the water pot lest they should find filth in it. Sometimes, when everything was ready for the dinner and the hungry children had taken their seats, the cruel disappointment would come, for the distracted mother had just detected the ubiquitous filth in the carefully-cooked dish. As a variety, Chathan would sometimes substitute human hair, charcoal and cowdung. Water mixed with turmeric and saffron—a vicarious offering for blood considered to be the most favoured drink of the denizens of the spirit world—would be found spilt here and there in the house. Squares, circles and other patterns by which Indian women usually decorate their floor were also drawn in the various rooms of the house. To expel the devil, an image of St. Anthony, reputed to possess power over spirits, was brought into the house. But an hour had not elapsed before it was found lying in the compound. One day Chathan developed into an incendiary. Jackets, petticoats, straw and dried

palm leaves were suddenly found to be in flames. Inanimate objects seemed to quicken into life; pictures and mirrors were heard to fall from the pegs on which they had been hung and broken. Pottery, china and cooking utensils of bell-metal, brass and copper, were violently flung about the rooms.

On the day when I paid my visit to this haunted house the Chathan had become more aggressive than usual. He had removed the bangles from the wrist of the baby of the house, broken them and concealed them under the roof. An old lady of the house had become the target of Chathan's violence. Stones, small pots, dried cakes of cowdung, had been hurled at her in the morning. I was standing near with my back to her when suddenly she uttered a cry of pain, and I turned only to see a brass lamp falling to the ground after hitting her. I was told that that particular lamp had been kept locked in a trunk. But locked doors and boxes were not proof against Chathan. The only way of preventing him from opening boxes and scattering their contents was to tie them with ropes or strings. For the devil has no thumb, and without it he cannot untie the knot.

My visit was very opportune in that I was able to witness one of the methods of expelling the devil. It is the peculiar characteristic of the people of Malabar not to do anything without ascertaining the will of the gods about it. The astrologer occupies a unique place in Malabar society. He is in demand everywhere and for everything. He had been consulted. From the stars the astrologer had been able to read the particular Chathan (for according to popular belief they are twelve brothers) who had chosen to visit the house, the reasons for his visit, the person against whom he had been set in motion (the Chathans have no will of their own and are the absolute slaves of those who have them in their power), and the person who could expel him from the house.

Here we come upon another feature of Malabar society. Certain families are considered to possess influence over spirits. Of these, the most notable are the Nambudiri families of Kallur and Kattumadam, and the Parayans of Tolanur. The former are at the top, the latter at the bottom, of the social ladder; the former are the descendants of the Aryan immigrants to South India, the latter of the aborigines; the former use their power for good, for casting out devils, and charge no fee, the latter generally for blackmail and mischief.

In this particular case the stars pointed to Kallur as the person who could deal with the devil. But he could not come on account of a pollution. So he wrote a letter in the following words: "Kallur Nambudiripad to Kuttichathan" ("It is our desire that you do desist from all your activities till we come to you"). This letter was solemnly read by the head of the house after purifying himself by a bath. But it had no effect. At last a disciple of Tolanur was called in, and I am told that the trouble has ceased.

Such incidents as these are by no means uncommon in Malabar. In almost every village, folk can tell you of some person who had fallen a victim to Chathan's pranks or point to you some house deserted on account of his activities. This Chathan, a faithful and obedient slave to his master but a perpetual worry and nuisance to those whom he may be sent against, is a god worshipped by the Pulayas, Parayans, and other castes, low in the social scale. His worship by the Brahman Nambudiris incidentally illustrates the process by which South Indian demonology has grown up; the gods of the vanquished aborigines have been recognised and given a place by the successful invaders, but as devils and spirits.

But it is the peculiar function and glory of science to reveal hidden causes, to offer a rational explanation for what at first sight or to laymen appears unaccountable. In this way diseases like epilepsy which had been regarded as due to "possession" have been satisfactorily explained. It is quite probable that this Chathan may, on a fuller and more careful investigation, be found to be some

inmate of the house suffering from a nervous affection which does not paralyse but only perverts the senses and which makes the patient feel delighted in handling things one normally detests and abhors. In the rare instances in which people had set themselves to watch and discover, they had been able to detect a human agency, but acting under the influence of spite and revenge. In these cases, however, the activities had stopped short at the threshold of the house and had never extended or penetrated into the inner apartments. But in the particular instance I have described, and in numerous others of which I have enquired, these motives have to be ruled out, because these evil deeds must have been wrought by an individual who is a member of the family, who has, therefore, the free run of the house and who stands to lose, like other members, by his or her destructive frolics. Nobody has seen the actual falling or flying of things; they do not seem to fall when any one is present in the room; it is the thud or the clang of the fallen article that draws one's attention to it. Wider enquiries, moreover, may elicit the fact that in 50 cases out of 100 these things are first seen or reported by one and the same individual. But so great is the faith in things unseen, that the mere suggestion of Chathan precludes all observation or even the desire to observe on the part of those who have the best opportunity to do it. Further, the opening of steel locks with which Chathan is credited, runs counter to another superstition that devils and spirits cannot and will not come near iron. The locks and fastenings do not bear any marks of violence. It is obvious they must have been opened in the usual way. None but an inmate of the house, whose *bona fides* is above suspicion, and who has free access to the keys, could have opened the cupboards and trunks. Now, it is always more difficult to untie a knot than to open a lock with a key, and if he or she attempts to do it it exposes the author to the risk of detection. Hence the fiction has been invented that Chathan has no thumb and he cannot meddle with things that have been bundled up and carefully and tightly secured by a rope or string. It is also probable that women are more liable to this disorder than men. For it is only one who moves freely in the kitchen that can easily and without being noticed defile drinking water and the well-cooked food. In Indian households cooks are seldom employed, and the women of the house prepare the food.

It will be interesting to know whether such phenomena as these have been noted among other peoples and races.

K. V. KRISHNA AYYAR.

### Anthropology, Physical.

Young.

#### **The Problem of the Racial Significance of the Blood Groups.**

*By Matthew Young, M.D.*

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In recent years much attention has been directed to the possibility of using blood group differences as an aid to the elucidation of racial origins and relationships and to the differentiation of mankind into racial types. Many data have been published giving the percentages of various peoples in different parts of the world that fall into the four blood groups that have been described and are now generally recognised. In different geographical areas or among different populations considerable variation in the relative proportions of the four blood groups has been found, and attempts have been made to classify mankind into racial groups on the basis of these percentage differences or on factors derived therefrom. The evidence so far available, however, presents difficulties of interpretation. The relative distribution of the blood groups cannot apparently be brought into agreement either with that of the racial types hitherto recognised and differentiated on the basis of physical characters or with geographical conditions. In view of this apparent lack of harmony, it seems to be of interest to bring under review and to analyse the data relative to proportions of the blood groups in the

inhabitants of different areas, that have recently accumulated, with the object of trying to assess the importance and the reliability of this characteristic as an additional aid to racial differentiation and classification.

As is now well known, the classification of the blood into groups was due primarily to Landsteiner (1901), who, having found that the serum of certain human bloods agglutinated or clumped the cells of certain other human bloods, proceeded to classify bloods into three groups based on the agglutination reactions which he obtained. Subsequently, it was found by other observers that there were exceptional cases that apparently did not fall into these three groups and Jansky in 1907 made a definite classification into four groups which he referred to by numbers I, II, III and IV. Moss, a short time later, described a second numerical grouping in which numbers IV and I in Jansky's classification were interchanged. Landsteiner suggested that the groups should be classified according to the iso-agglutinin content of the cells instead of numerically. Thus we have the four groups usually defined as *O*, *A*, *B* and *AB*, the first of which consists of bloods wherein the red cells carry neither of the agglutinable factors or agglutinogens; the second in which the red cells carry the *A* factor alone; the third in which they carry the *B* factor alone; and the fourth in which both the *A* and *B* agglutinable factors of Von Dungern are present. At various times, anomalous bloods have been described which do not appear to fall into any one of the four groups mentioned; but most observers seem now to be more or less in agreement in the view that these apparently exceptional cases can be explained as usually due to pseudo-agglutination, low agglutinin-agglutinin titre, failure of complete adsorption in adsorption experiments or somewhat allied conditions and that the four well-recognised groups are sufficient to include all cases (*see* Dyke, 1927).

Interest in the subject of the application of blood grouping to anthropology and racial differentiation was first aroused by the work of L. and H. Hirschfeld (1919), who showed that the percentages of the blood groups varied in different peoples and that the variation was related to geographical distribution. They found that the proportion of agglutinin *A*, characteristic of Group II or Group *A*, appeared to predominate greatly over the proportion of agglutinin *B* in European peoples, whereas the factor *B* predominated over the factor *A* in Asia and Africa. Factor *A* decreased in passing from west to east and *B* decreased in passing from east to west. These conclusions were drawn from the examination of about 500 members of each of 16 different nations or peoples. They classified these peoples on the basis of a "biochemical index" or racial index, namely, the ratio of the percentage of the *A* factor to the percentage of the *B* factor or  $\frac{\%A + \%AB}{\%B + \%AB}$  into three groups: (1) European, (2) Intermediate and (3) Asio-African. The races with an index higher than 2.5 were assigned to the European type, those with an index of less than 1 to the Asio-African type, whereas those with an index between 1.3 and 1.8 were classed as intermediate. The data furnished by more recent observations on different peoples have shown that the chosen limits of this subdivision are quite arbitrary as intermediate values of the index occur, and the values now available for the different peoples seem to form a more or less continuous series. Steffan (1926) calculated Hirschfeld's "biochemical index," which he describes as the biological race index *A* (or the Atlantischer index), for a long series of blood group records of different peoples, including many not available to the Hirschfelds. He also tabulated the values of an index *G* (the Gondwanischer index), the reciprocal of the *A* index. Having indicated the location of the populations under consideration on maps, he drew curved lines connecting peoples with corresponding values of the indices, inserting broken lines to indicate the probable distribution of intervening values. The maps resemble contour maps and seem to

show that the centre of distribution of races with a high *A* index or with a relative preponderance of persons falling into the *A* or *II* blood group was in the north-west of Europe, whereas the centre of distribution of the *G* type was in China, in the vicinity of Peking. Assuming that races with high *A* and *G* values were primitive, Steffan then discussed the probable migratory history of these. Ottenberg (1925) also made a study of more material than was available to the Hirschfelds and considered that, so far as blood groups are concerned, the races represented may be readily classified into six strikingly different types, namely, (1) European, (2) Intermediate, (3) Hunan, (4) Indo-Manchurian, (5) African-south-Asiatic and (6) Pacific-American. Ottenberg made the reservation, however, that he put forward this classification as merely provisional and as based on the scanty knowledge existing at the time.

Both Bernstein and Snyder have pointed out that the biochemical index as used by the Hirschfelds is not an adequate differential criterion mathematically, and that the actual frequencies of the hereditary factors, *A* and *B*, in a population, frequencies which can be calculated easily for any population wherein the percentages of the four blood groups are known, should be used. Snyder (1926) presents his data in the form of a chart in which *p* and *q*, the frequencies of the factors *A* and *B* in the different peoples, are the coordinates. In this correlation table the peoples investigated appear to fall into more or less natural groups. On the basis of the relative proportions of the *p* and *q* factors, the various peoples were divided arbitrarily into types. These types are in substantial agreement with Ottenberg's, although there has been a redistribution of a few of the component members of the groups. They are seven in number, as the Australian type has been set apart from the Pacific-American, and are as follows: (1) European, (2) Intermediate, (3) Hunan, (4) Indo-Manchurian, (5) Africo-Malaysian, (6) Pacific-American and (7) Australian. Snyder emphasised the fact, however, that it is not implied that because two peoples occur in the same class they have the same racial history, but only that they contain approximately similar amounts of the *A* and *B* factors. He states that the types are purely arbitrary, are subject to revision, and merely serve as a working basis for racial classification on serological grounds. Wellisch (1927) calculated a modified race index, the Bernstein index, *i.e.*, the ratio of the percentage of *p* to the percentage of *q*, or *p/q*, and found that the use of this index made comparatively little change in the relative positions of the various peoples arranged in accordance with the values of the Hirschfeld index.

The expectation that the proportional distribution of the blood groups in a people may shed light on its racial origin is not unreasonable, as it is now generally agreed that the factors *A* and *B* are inherited in a typical Mendelian manner. For some time it was commonly believed that the factors were inherited as two independent pairs of Mendelian factors, but recent observations suggest that the mode of inheritance may be more adequately explained on the hypothesis that the inherited factors are a series of three multiple allelomorphs. Moreover, evidence has been furnished by certain observers, including Ottenberg (1923), which tends to show that the proportions of the four blood groups are remarkably stable in a given population, provided no outside admixture occurs. They may apparently remain the same for an indefinite number of generations.

Records of the percentages falling into the four blood groups in the various peoples throughout the world are steadily accumulating. Many of these have been collected from different publications by Snyder (1926), who provides a long list of data, including some original observations. These data and other data collected from various sources, principally from Scheidt's memoir (1927), provide the material for the present inquiry into some of the points at issue in regard to the racial significance of the blood groups.



The hypothesis has been advanced by the Hirschfelds that originally all bloods belonged to Group *O* and that the *A* and *B* factors arose as mutations, factor *A* in the European zone and factor *B* in the Asiatic zone, and that the population groups with intermediate proportions of these factors have arisen by admixture or commingling of peoples bearing the respective factors. Some such hypothesis might certainly readily explain the predominance of the *A* factor in western Europe, the *B* factor in Asia and the intermediate proportions of the two factors in the intervening area. Snyder (1926) finds in the relatively high proportion of Group *O* in the pure-blooded American Indians, whom he selected as a people specially suitable for the study of racial admixture, support for the view that originally human blood was all of Group *O*. He contrasted the proportions in the pure bloods with those in Indians with white admixture and found the proportion of Group *O* considerably reduced and the proportion of Group *A* increased and that the transfer from Group *O* to Group *A* seemed to correspond with the amount of admixture. Coca and Diebert (1923), in view of the high proportion of Group *O* in the American Indians, suggested that this people became separated from the human family before the appearance of the iso-agglutination factors in the blood and that the existence of these elements in the blood of some of the Indians examined by them was due to the admixture with white people. The acceptance of such a possible date of appearance of the factors in the human stock is, however, rendered less probable as a result of Landsteiner and Miller's (1925) recent observations, which appear to show that factors *A* and *B*, practically identical with those of human beings, occur in the blood of anthropoid apes.

A survey of the available data reveals the fact that the people with the highest percentage of Group *O* are the North American Indians, in whom, among the pure bloods, a percentage as high as 91·3 has been recorded by Snyder. In other groups of North American Indians, Group *O* forms 78 per cent. of the total. Among Polar Eskimos, Henbecker and Pauli (1927) have found the proportion of Group *O* to be 81 per cent. of the total, and these authors suggest that the pure Polar Eskimos were probably also all of Group *O* as the small proportion of persons falling into the groups other than *O* were generally shown to be half-breeds. In the remaining data, percentages of Group *O* equal to or exceeding 50 have been recorded for relatively few population groups. Among these are the Filipinos with 64 per cent., the Australian aborigines with 56 per cent., the Icelanders with 55·6 per cent., the Melanesians (N. Guinea) with 53·7 per cent., the Bogobos (Philippines) with 53·6 per cent., the South African natives with 52 per cent., the white Australians with 52·6 per cent., the Swedish Lapps and Igorotos (Philippines) each with 51 per cent. and Brazilians with 50 per cent. Recent data from the Philippines for certain relatively unmixed tribes suggest that the proportion of Group *O* is not so high there as was formerly believed.

It has been suggested by Snyder (1926) that the majority of the peoples with a proportion of Group *O* exceeding 50 per cent. are island peoples or peoples living in regions more or less isolated, and so presumably less liable to admixture. This description is certainly applicable to many of the peoples indicated, but some other populations showing amongst the lowest proportions of Group *O*, namely, the Ainu with 19 per cent., South Koreans with 19·9 per cent., North Japanese with 29 per cent., and North Chinese (Shantung) with 21 per cent., might be similarly described. The proportion of Group *O*, namely, 52 per cent., in Australian whites is based on nearly 1,200 observations and so may be considered fairly reliable. The proportion of the same group estimated for the consecutive series of 100 observations of this total has been shown by Tebbutt (1923) to range in value from 46 to 65 per cent. This divergence indicates the amount of variation that may be found in a fairly homogeneous population when the number of observations does not exceed 100.

In view of the relatively high proportion of Group *O* in the white Australian population, which cannot be described as being other than of comparatively recent foundation and mainly, though not entirely, of British origin—Carr Saunders (1927) states that well over 90 per cent. of the population of Australia has British or Irish antecedents—it is evident that not too great stress can be laid upon the significance of a proportion of Group *O* just exceeding 50 per cent. in many of the islands or relatively isolated regions described by Snyder as indicative of lack of admixture therein.

Though information regarding the blood groups of the various peoples throughout the world is steadily accumulating, the percentage distributions of the blood groups published are based, in many instances, on relatively small numbers of observations. It is obvious that, in view of the possible errors of sampling under such circumstances, apparent differences may not be statistically significant; in other words, the numbers available may not be sufficient to indicate that any importance can be attached to the difference shown. This defect may be illustrated by the application to a selection of the data of the method of analysis devised by Prof. Karl Pearson (1911) to ascertain the probability that, given two frequency distributions of phenomena, one distribution is really different from the other to a greater degree than can reasonably be supposed to have arisen by the operation of chance alone. The formula used is given below.\*

Professor Pearson has used this method to determine whether various populations were differentiated with regard to hair colour. By this test, the proportions in the corresponding blood groups in samples of any two populations can be compared and the probability that such differences as occur may arise fortuitously can be determined. The test has been applied: (1) to a selection of samples of populations from the same nation or the same geographical region, and (2) to certain samples of populations that, according to present conceptions of racial relationships, apparently belong to different racial groups. The application of the test has revealed some rather interesting results. In the first place, comparing the blood group distributions in two samples of population from the same nation, it is found that in a number of instances among the available data there is a distinct differentiation—the differences are greater than are likely to arise fortuitously on the numbers examined. Thus the Germans† from Schleswig-Holstein differ significantly from the Germans in Leipzig, the Italians examined by the Hirschfelds differ from the Italians examined by Mino, the Americans examined by Snyder and Moss both differ from the Americans examined by Culpepper, the Hungarians examined by Von Jeney differ from the Hungarians examined by Manuila, the Roumanians residing in the mountainous regions differ from the Roumanians in the valleys (Popoviciu), both the Armenians examined by Parr and those examined by Altounyan differ from the Armenians of Kossowitch, the Japanese in the vicinity of Hida from the Japanese of Nagoya and of Niigata, the Ainu recorded by Ninomiya from the Ainu of Nakajima; one group of Philippine Moros diverges significantly from a second group, each comprising about 500 persons, though the

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$$\chi^2 = S_1^* \left\{ \frac{NN' \left( \frac{fp}{N} - \frac{f'p}{N'} \right)^2}{fp + f'p} \right\}$$

where  $fp$  and  $f'p$  represent the frequencies in corresponding groups of the two samples compared,  $N$  and  $N'$  represent the total numbers in the two samples and  $S_1^*$  represents the summation of the several terms. The required probability that the two samples are undifferentiated, *i.e.*, did come as random samples from the same population, may be found by looking up the value of  $P$  corresponding to the ascertained  $\chi^2$  and  $n'$ , the number of classes, in the tables given on pp. 26–29 of Pearson's Tables for Statisticians and Biometricians.

† The peoples whose blood groups are compared, the names of the observers, the number of observations on which the comparisons are based, and the resulting values of  $\chi^2$  and  $P$ , with comments thereon, are shown in Table I.

groups presented a very close resemblance in physical characters. The lack of concord in distribution of blood groups in groups of Moros indistinguishable as regards physical type was first pointed out by Grove (1926). On the other hand, neither a sample of the English population nor of Australian colonists can be regarded, on the numbers available, as differentiated in respect of their blood group distribution from the pure-blooded Australian aborigines, the American negroes are not differentiated from Russians, though they appear to diverge significantly from Senegalese, nor are Spanish Jews (Hirschfeld and Hirschfeld) differentiated from Chinese, nor Manchus from the Ainu, nor Viennese from the Japanese, nor Egyptians from the Ainu, nor Swedes from Lapps, nor Germans in Berlin from Turks, nor Germans in Kiel from Bulgarians, nor Ukrainians from South Koreans. The lack of differentiation between the blood group distributions in Germans and Turks and Bulgarians respectively is of interest as in Snyder's serological classification of the races, the Germans fall into the "European" type, whereas the others are described as of "Intermediate" type.

Unfortunately, there is no general agreement among anthropologists in regard to the racial groups into which mankind may or should be divided on the basis of physical characters, among which, skin colour, form and distribution of hair on scalp and body, stature, shape of head and characters of face, nose and eyes are those usually relied upon. The particular physical traits or the number of traits that are to be taken into account in differentiating races is still a matter of individual opinion. Thus the term "race" may be restricted to the three broad groups of people distinguished by degree of pigmentation, white, dark and yellow, or these may be subdivided by taking into consideration other characters such as type of hair or form of head. The limits of the racial groups will obviously depend on the characters selected as criteria. A broad, general classification of racial types based on physical characters which has been found sufficiently comprehensive is the following: (1) Australoid, (2) Negroid, (3) Mongolian, (4) Central or Alpine, (5) Mediterranean and (6) Nordic. The main point of contrast between this racial grouping based on physical characters and Snyder's serological grouping is that, whereas Snyder's scheme proposes no subdivision of the western European type, which is a mixture of the three types, Central or Alpine, Mediterranean and Nordic, it differentiates the groups Hunan, Indo-Manchurian and Pacific-American, which, in large part at least, would fall into the group broadly classed as Mongolian in the first scheme. This subdivision is of special interest, as, according to Haddon, there is now a Mongol nation and not strictly speaking a Mongol "race," but the term "Mongol" has become established by constant usage for the group of peoples. Race names such as Nordic and Alpine have been described as merely convenient abstractions helping us to appreciate broad facts. Wellisch (1927) seems to suggest that, on the average, the Nordic, Mediterranean and Alpine races in Europe show a gradually decreasing Bernstein index, ( $p/q$ ), indicating a fall in the relative proportion of Group A in that sequence. It is doubtful, however, if the differences pointed out by him can be considered significant. Scheidt (1927) has made a detailed analysis and comparison of the proportions of the blood groups in many different peoples. In numerous cases he has calculated the standard errors of the percentages in the different groups and has thus been able to determine if differences in corresponding groups in various peoples may be considered significant on the figures available. He found evidence that preponderatingly Nordic people (in Angeln and Pellworm) could be distinguished by a greater A frequency. It appeared considerably more uncertain whether the Alpine populations (Alpine being used as indicative of area occupied and not in an ethnological sense) were distinguished from them by a smaller A frequency, particularly if it is accepted that the population of Peterstal (in the Black Forest) in its other racial characters stands nearer to the

Alpine population than perhaps the lower German peoples. He states, further, that the comparisons of the Italian groups with the north European groups show only a few reliable differences. Results from a comparison of certain European peoples regarded as mainly belonging to these different racial types seem to be somewhat equivocal. Taking the Swedish percentage distributions recorded by Lindberger as representative of the Nordic type, the distribution of the blood groups in this "race" is not differentiated from that shown in a group of about 500 Alpines in the Black Forest (the Alpine group has the higher proportion of *A*), but is differentiated from Serbians, who may be regarded as mainly of Alpine type. The distribution of the blood groups in Swedes (Lindberger) is significantly different from that in one group of Italians examined by Hirschfeld, but not from that of another group examined by Mino. Hirschfeld's data appear to have been derived from all over Italy and Mino's mainly from the north of Italy, but they may be taken generally as more or less representative of the Mediterranean type. The distribution of the blood groups in Swedes (Lindberger), representative of the Nordic type, is significantly different from that of the Germans in Schleswig-Holstein, who are described also as being mainly of the Nordic type. This is the district in which the Nordic type is said to be best preserved in Germany. The distribution of the blood groups in the Serbians (the Hirschfelds) differs significantly from that in both the Italians examined by the Hirschfelds and in those examined by Mino; whereas the distribution of the blood groups in the Sardinians, who may be regarded as mainly of Mediterranean type, is not significantly different from that of Slovaks in Roumania, who may be considered as predominantly of Alpine type. From these results it is obvious that, with the data available, no very definite inferences can be drawn regarding the existence of any significant or characteristic differentiation in the blood grouping of the three European types, Nordic, Alpine and Mediterranean, although, according to many anthropologists, these three types represent three very clearly defined and distinctive races, which appear almost as much differentiated from one another in regard to physical characters as any one of them is from the rest of the groups of the human family, Australian, Negro and Mongol.

The principal points that appear to emerge from the analysis and study of the data may now be summarised briefly and discussed. It seems to be clearly established that whereas the relative proportions of the blood groups vary considerably in the different peoples examined, there is a definite preponderance of Group III or the factor *B* in the peoples of the eastern Asiatic zone and a relative excess of Group II, indicating a preponderance of the *A* factor, in the peoples of the western European zone. There appears to be some evidence in favour of the hypothesis that human blood was originally all of Group *O*, or devoid of agglutinogenic factors, but when or where the separate mutations resulting in the appearance of the *A* and *B* factors in the human stock occurred and whether they were single or repeated is still a matter of pure conjecture. The maximum concentration of the respective factors *A* and *B* in west Europe and China or India would suggest that the points of origin were located in these areas. The relative paucity of the *A* and *B* factors in certain "races," such as the American Indians and the Eskimos, has been attributed to the separation of these peoples from the primitive stock before the appearance of the agglutinogenic factors; but the lack of differentiation between the blood group distributions of Australian colonists and full-blooded Australian aborigines, the latter of whom may be regarded as undoubtedly the most primitive race now living and who represent the survival with comparatively slight modifications of perhaps the primitive type of the species (Elliot Smith), suggests that no such inference can be drawn from the Australian data.

*(To be continued.)*

## REVIEWS.

India : Ethnology.

Viswanatha.

*Racial Synthesis in Hindu Culture.* 117  
By S. V. Viswanatha. Kegan Paul, Trench, Trübner & Co. 1928. Pp. vii + 234. With a map. Price 10s. 6d.

The races dealt with in this work are (1) the Āryas, and (2) the non-Āryan peoples of India. We are not told to what race the Āryas belonged; but, after discussing certain views regarding their origin, Mr. Viswanatha considers that the idea of an Āryan invasion of India must be given up; and he would place "the cradle of the Āryas" in the Himālayan region, "roughly extending from the "valleys of the Seven Rivers to the place "where the Ganges and the Jumna are "seen to have their rise." Of the non-Āryan element in the population of India, we are told that it "has run in two main "streams that may be designated the "Kolarian and the Dravidian." This does not help to increase our store of knowledge. The title of the book would lead us to expect an account of the synthesis of these races, as such; but the contents treats of the fusion of the religious and social cultures, and other subjects.

The opening words of the Introduction—"The ancient history of India teems with "illustrations which go to prove its fundamental unity . . ." perhaps furnish a clue to the objective which the author had in view. If so, the conclusions drawn in Part V were only to be expected, *e.g.*, that "the divergent cults and creeds sank "their prejudices and differences to realise "the unity and the peace of her past," and that "a spirit of conciliation and "compromise seems to have pervaded the "relations of the various peoples of India "even from the beginning of her history." The survey intended to establish these conceptions is divided into three parts, viz. : (1) the Age of the Mantra; (2) the Later Vedic and Heroic Period, and (3) the Epoch of Modern Hinduism. This last period seems to be what is described on p. 8 as the Period of Buddhism, the Smritis, the Purāṇas and the *Sāṅgam* Texts of South India, which can not correctly be described as "the epoch of modern Hinduism," more especially as we are told that the scope of the survey extends only to about the fourth century A.D. The limits of the Vedic and Epic periods are still quite indefinite. To the compilers of the *Rigveda*, however, the peninsula of India would appear to have been a *terra incognita*. While many of the rivers of Northern India are named therein,

the Narmadā is not mentioned, and even the Vindhya mountains seem to have been beyond their ken. It is not yet known when the Āryas of the early Sanskrit texts penetrated the peninsula towards the south. The earliest mention of their expansion eastwards, towards North Bihār, hitherto found is the story in the *Śatapatha Brāhmaṇa* referred to on p. 74. In the age of the *Brāhmaṇas*, for which various estimates have been suggested, and which was probably anterior to the rise of Buddhism, it is at least unlikely that "Āryan" culture had passed the Vindhyas. In the *Mānavadharmasāstra*, which is ascribed approximately to between 200 B.C. and 200 A.D., the southern limit of *Āryavarta*, or "the abode of the Āryas," is given as the Vindhyas. Even Rājasekhara, who wrote *circa* 900 A.D., in his *Kāvya-mīmāṃsā* (chap. 17) makes the Vindhyas the southern boundary of *Āryavarta*. There is no historical foundation for the statement that the *Rāmāyaṇa* discloses the migration of the Āryas beyond the Vindhyas "to "colonise the lands inhabited by the non-"Āryans of the South." The legend of Rāma's adventures, moreover, professedly relates to a period many centuries anterior to even the "Great War" of the *Mahābhārata*.

Mr. Viswanatha tells us that the sources of his information are chiefly works of Sanskrit literature, and that non-Āryan sources are rare "and require dexterous handling." A note of caution might also be sounded in respect of the Sanskrit literature, having regard to its provenance. Many scholars who have studied the same sources will find it difficult to accept all Mr. Viswanatha's conclusions; in fact, they will find evidence of the age-long conflict between the Brahmanical and non-Brahmanical cultures, and of the important part that religious antagonism has ever played in the historical events of the country. Racial and tribal distinctions have been largely effaced by long centuries of intercourse and cross-breeding, while religious differences have survived. Where we do notice attempts to create religious toleration and harmony during the early period dealt with by the author, it will generally be found to have been due to the influences of Buddhism and Jainism. The widest political unity was attained under the Maurya dynasty, but even the great Aśoka did not unify the whole of what we now call India; and, as Professor Jouveau Dubreuil has shown, none of the kingdoms of the Dekkan remained in the possession of the Guptas,

who established the next great empire in the fourth century A.D.

A few words should be added regarding the map of Ancient India, which has not been carefully drawn. The positions of nearly all the countries marked in N.E. India are incorrect. For instance, Aṅga lay mostly south of the Ganges, about modern Bhāgalpur, and Paundra to the E. and N.E. of Aṅga, between it and Kāmarūpa. Vāṅga was eastern, and Suhma, so far as we can judge, was south-western Bengal. Magadha did not stretch across the Son to the west. Mithila lay to the east of the Gandak, between it and the Kosi. Vidarbha was what we now call Berār, and not Gujarāt. Southern Kosala comprised the country about the modern districts of Bilāspur and Raipur, and did not lie, as marked, to the south of Allahabad and west of the Son. Andhra has been shown in two different positions. Mohenjo Daro has been marked quite out of place.

C. E. A. W. O.

Africa: Folklore.

Trautmann.

*La Littérature populaire à la Côte des Esclaves. Contes. Proverbes. Devinettes.* Par René Trautmann, Médecin Major de 1<sup>re</sup> Classe des troupes coloniales. Paris: Institut d'Ethnologie. 1927. Pp. vii.-105, cartonné toile. 30 fr. (étranger, 45 fr.)

Despite the charm with which these stories have been translated, we confess to a feeling of disappointment. The earlier publications of the series, entitled *Travaux et Mémoires de l'Institut d'Ethnologie*, of which this is the fourth volume, have led us to expect something more than a medley of folk stories, unrelated to any sociological content whatever. They are, moreover, classified under moral categories, such as Charité, Pitié, Indulgence, Bien-faisance, and so on, which are definitely European classifications, and, as such, out of place in a scientific collection of folktales from West Africa.

Without their sociological background and without any indication of the part which these stories play in the life of the people, they are shorn of more than half their value. This is not to say that they have no value at all; for, indeed, as M. Delafosse justly points out in his brief preface, they give "une idée à peu près exacte de la mentalité des habitants de ce pays." Yes, but how much more luminous they would have been, how much more valuable a guide, if we had only been given something more than a mere translation! It is beside the point for M. Delafosse to write: "Il a redouté d'altérer, par une intervention personnelle que celle qu'elle fut, le caractère propre des

"recits. . . . Il a voulu laisser aux "lecteurs toute liberté de tirer de leur "lecture les réflexions qu'elle leur "inspirera." That is all very well, but the reader has no criteria, no standards: how can he form any judgment which is not subjective?

The preface, as one might expect, suggests that African mentality is different from our own, and that these translations will enable us to analyse this alien mentality and to observe the operation of unfamiliar logical processes. If, however, the stories reproduced thus baldly form any index to native mentality, one is irresistably forced to the conclusion that there is no difference whatever between their mentality and ours and nothing to justify a depreciation of their logical conceptions. There is nothing prelogical, for instance, in the comparison of knowledge to a baobab tree on p. 59; and read where one will, one is confronted on every page with very definite evidence invalidating the theories of the French psychological school.

The proverbs are interesting and exhibit the same universality as one finds among proverb-makers all the world over; but probably they and the riddles lose even more than the stories by translation, in that their essential assonances and *doubles entendres* cannot be preserved even in French, the most flexible of European languages.

Footnote 2 on p. 2 appears to be incomplete. (Calalou) "Plat indigène composé . . ." The reader presumably has full liberty "de tirer de leur lecture les "réflexions qu'elle leur inspirera." Would it be unkind to retort that this volume is *un plat indigène composé . . . ?* And we hope that some day Dr. Trautmann will both tell us what *calalou* is, and will also furnish the contextual background of the tales which he has so delightfully rendered.

J. H. D.

Africa: Art.

Nuoffer: Vatter.

*Afrikanische Plastik in der Gestaltung von Mutter und Kind.* Von Oskar Nuoffer. Dresden: Carl Reissner. Pp. 80, 75 plates and 24 figures in text; 9 in. × 6 in.

*Religiöse Plastik der Naturvölker.* Von Ernst Vatter. Frankfurt a/M. Pp. 194, 101 illustrations, 10 in. × 6½ in.

The subject of African art has only begun to attract serious attention within the last few years. While art was regarded solely from the standpoint of classical and European conceptions of beauty, it was inevitable that, as Professor Vatter says, "die bildnerischen Erzeugnisse der Naturvölker . . . nur eine seltsame, viel leicht nicht uninteressante Verirrung des Menschengestes bedeuten konnten."

The interest recently awakened in this subject was indicated, a few years ago, by an exhibition of Negro Art in London, but it would seem to be still greater on the Continent, if one may judge from the two books before us. Herr Nuoffer's [why, one may ask in passing, is he called Oskar on the title page and Otto on the dust-cover?] forms part of the series, "Religiöse Kunst," the rest of which, with one exception, is devoted to European art. It is a very thorough examination of the way in which the "Mother and Child" motive has been treated in West African sculpture; no eastern examples are recorded—at least, nothing farther east than the Lualaba-Kasai area. The very numerous figures of this kind from the Congo are interpreted as fertility-charms; perhaps the same idea is present in all, though partly or wholly lost sight of where art is more highly developed and cultivated more or less for its own sake. It has, in the writer's opinion, reached its highest point in the grass country of the Cameroons, where it is still flourishing. He quotes from Emonts a description—interesting from more than one point of view—of a sculptor at work, the Chief of Babanki, who, in fact, owes his position to the admiration excited by his skill. The art of Yoruba and Dahomey, probably at its best but little inferior, shows signs of decadence. Sculpture in stone is rarely met with: an interesting example from Sherbro is figured on p. 57, and two grave-monuments from Ambrizette and neighbourhood on p. 67; these last are clearly due to European influence. The carving of the figures in relief, not met with elsewhere, seems to have presented peculiar difficulties, and the artist has finished off the heads in the round. The influence of European art does not seem to have been very extensive; it appears principally in the region of the Lower Congo, though Plate 45 shows a remarkable Madonna and Child, in wood, from Senegambia. One may doubt whether the author's conclusion, that the absence of feeling is an essential characteristic of African art, and that any change in this respect must destroy it, will be universally accepted.

Professor Vatter treats the subject on more general lines, and does not confine his survey to Africa. He stresses especially the religious character of all primitive art, manifested in the fact that "der Auflösung" und Zersetzung der primitiven Weltanschauung unter dem Einfluss der europäischen Zivilisation der Zusammenbruch der Kunst auf dem Fusse zuzufolgen pflegt." It is symptomatic of an age which has lost faith in its own ideals that it should overrate (while not always

understanding) the merits of barbaric art. This very thoughtful and suggestive study cannot be adequately dealt with in a short notice. As usual with recent products of German typography, the printing and illustrations are of the highest excellence.

A. WERNER.

Egypt: Archæology.

Moret

*The Nile and Egyptian Civilization.*

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By Professor Alexandre Moret. London: Kegan Paul, Trench, Trübner & Co. 1927. Pp. 497. Price 25s. net.

This book suffers from a too enthusiastic foreword, where encomiums are so lavishly applied that ordinary praise must appear dim and paltry in comparison. The misfortune is that such uncritical laudation is apt to produce prejudice, which is particularly unfortunate in the case of a fine piece of work such as Professor Moret's book. Professor Moret has used all the literary evidence known, and from these documents has compiled his sketch of the long history of Egypt. In the short space of less than five hundred pages he has compressed an account of the country and of the social organisation and intellectual life of the different periods as well as of the history of events. He has even found room for a chapter on sources and chronology. But though the matter is necessarily given in so small a compass, Professor Moret has succeeded in making it interesting, and this is a triumph in itself. The main fault of the book is that it omits almost entirely the whole of the material culture, of which so much is known, and which gives the human interest to all studies of the ancient past. Archæology is often a sealed book to the "mere linguist," yet it is to archæology that we owe considerably more than half our knowledge of movements of peoples, as well as their state of civilisation, even in historic times. A work which ignores the culture of the nation of whom it treats cannot be considered complete, for the outlook is not sufficiently extensive. But as a very complete history taken from the standpoint of documentary evidence only, this book can be relied on as being full of accurate information set out in a manner which makes it a pleasure to read.

M. A. MURRAY.

Malay Peninsula: Ethnology.

Evans.

*Papers on the Ethnology and Archaeology of the Malay Peninsula.*

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By Ivor H. N. Evans. 8½ in. × 5½ in. Pp. x + 164; xliii plates. Cambridge: University Press, 1927. Price 15s. net.

The twenty-six papers contained in this volume fall under four main heads, viz., the Pagan Races, Malay Beliefs, Malay and other Technology, and the Archæology of



the Malay Peninsula. Most of them have already appeared in the *Journal of the Federated Malay States Museums* or the *Journal of the Royal Asiatic Society, Straits (now Malayan) Branch*, but they deserved to be reprinted in a collected form. The hitherto unpublished sections, which are concerned with Neolithic implements, cave exploration (including some Palaeolithic finds), and early bronze and iron tools, constitute an important contribution to our knowledge of the prehistoric inhabitants of the Peninsula.

The first paper in the book contains a vocabulary of the dialect of the Negritos of the Trang district (Lower Siam), which

has much in common with the dialects of the Negritos in the British sphere to the southward. The author has compared the Trang words with words of these other dialects recorded in Skeat and Blagden's "Pagan Races of the Malay Peninsula," ii, 509-764, and has given references where they agreed. To these I may add (p. 9) "cook, to," R 139; (p. 10) "jews' harp," J 6; "leg, below knee," C 5; (p. 11) "piss, to," U 27; "prawn," P 207; "rice, cooked," C 237; "sky," T 116. "Coconut," *tung* (p. 9), is probably the Khmer *doung* (spelt *tung*), with the same sense. The word "sweet" (p. 11) is a misprint for "sweat." C. O. BLAGDEN.

## CORRESPONDENCE.

### Ethnology.

#### The Children of the Sun.

To the Editor of MAN.

SIR,—Dr. Hutton and I have carried on a desultory correspondence in MAN for some time past, the main theme of which has been "The Children of the Sun." In MAN, 1927, 81, Dr. Hutton made the statement that "it is a commonplace that identical results may spring from entirely different origins." I replied (MAN, 1927, 147) "I know that (this process) is a commonplace of anthropological speculation, but let us have a real instance of it." The result has been to produce some instances. (MAN, 1928, 65). These are:—(i) the fire-piston, independently discovered, we are told, in Asia and, about 1800, in France; (ii) the bull-roarer is said to have been discovered independently by Mr. N. W. Thomas, the Australians, and whoever else use it; (iii) the use of adrenalin as an arrow poison by certain Amazonian Indians; (iv) the use of suggestion in medicine and magic.

I do not propose to say much in detail about these instances, but rather to try to see whether anything can be learned from them. (iv) can, I think, be dismissed forthwith. It is on a par with the comment of another anthropologist, who suggested that Elliot Smith and I contended that the drinking of water came from Egypt. Suggestion is a natural psychological process that enters into the lives of all of us. It is no more an invention than is breathing.

(ii) is of little value. Most of us have swung bits of wood on the end of pieces of string when boys. The use of the bull-roarer is a serious matter to the Australian. It is bound up with a complicated ritual, and must be explained along with it. The bull-roarer must be explained as part of the ritual.

### Perry.

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(iii) is a better instance. It presupposes, however, the knowledge of the use of poisons on the part of the tribe. I hardly suppose that Dr. Hutton would suggest that these tribes independently invented the use of poisons for arrows and so forth. This is another example of the argument based on a variation in a craft. Because the pre-Columbian Americans grew maize, it is assumed that they discovered agriculture for themselves. It is equally legitimate to assert that they may have discovered maize because they were agriculturalists. Arguments based on the differences between the raw materials of an industry are worth little. I have small doubt that the use of iron was discovered independently, but by people who were metal-workers. It cannot be asserted too often that the knowledge of a craft is the important thing; the particular form that the craft takes is secondary. Pottery-making is more important than painted pottery. Differences in painted pottery are merely secondary differences. In the same way people who use poisons will perhaps hit upon similar materials. The thing that matters is the use of poison.

Admitting, for the sake of argument, that Dr. Hutton's facts are correct, that adrenalin was discovered independently, another important point emerges, which involves (i). In both these cases one of the two parties is the modern European, who lives in an atmosphere of inquiry. If, therefore, any people in any part of the world, or at any time, have hit upon a discovery, that involves some mechanical or other principle, it is practically certain that it will be rediscovered by modern peoples. For instance, certain anatomical knowledge possessed by the Egyptians, and lost for ages, was rediscovered in Europe during the last century.



This is inevitable. Many examples could be brought forward to illustrate the principle. But when I asked for "a real instance" of independent invention I was thinking of something far different. The point in question was the dual organisation, a form of social organisation that appears to be difficult of understanding to many anthropologists. This is a definite form of social grouping, usually complicated in its nature, as may be seen from the evidence collected in "The Children of the Sun." It is entirely inconceivable to me that this grouping could have been formed twice, for I can find no evidence whatever to suggest that possibility. It can be claimed that it may have happened twice, but that is of no use. We must build our theories on facts, and not on possibilities.

I mean by a "real instance" of independent development of culture, a case in which two peoples, not of modern Europe and its derivatives, have invented, discovered or devised the same cultural element, say, metal-working, pottery-making, weaving, polished stone implements or totemic clans. That is the kind of demonstration for which I sought. Similar modifications of arts and crafts are of secondary importance, for they can more easily come about.

It may be possible to accumulate a certain number of instances, of more or less validity. It is certain, nevertheless, that, in the vast majority of cases, the common possession of the same element of culture, by two or more peoples, is due to diffusion. The only possible working hypothesis is that discoveries and inventions take place once.\*

W. J. PERRY.

Ireland: Archæology. Macalister.

### The Sligo "Artifacts."

To the Editor of MAN.

SIR,—A few weeks ago, I received 123 a letter from the Editor of *Nature*, enclosing a communication from Mr. Reid Moir, and asking me if I had any comments to make upon it. I returned it, with some observations. Acknowledging these, the Editor informed me that he did not propose to publish the correspondence, and that he had sent back Mr. Moir's communication, along with my reply. Mr. Moir profited by certain hints in the latter to make some important modifications in his communication, and reissued it, thus emended, through the post as a flyleaf. My reply was thus prevented from reaching his public. But as the flyleaf has now been republished in MAN, presumably because it was necessary

\* I shall discuss this problem in greater detail in the near future.

to elucidate a reference in the communication of M. l'Abbé Breuil, I may ask permission to make a few supplementary remarks. My original answer to *Nature*, though unpublished, adequately served its purpose, and need not be repeated.

As M. l'Abbé Breuil most truly says, the date of these Sligo objects is a question of geology. And the verdict of geology against a Palæolithic date for them is absolute and insuperable. This is the opinion of every competent geologist who has visited the place. It is not too much to call it inconceivable that there should have been a Palæolithic dwelling-site at any of the spots where the specimens have been collected.

We might admit the possibility of their being artifacts of a later date. This, however, would not serve the contentions of Messrs. Moir and Burchell, who claim to have found *Palæoliths*, not merely artifacts in general. But, though I yield to none in respect for the name and the authority of M. Breuil, I find it easier to believe that these amorphous objects have been formed by some natural force, and that criteria of human handiwork pointing to an opposite conclusion are illusory, than that any community, of a stage of civilisation later than Palæolithic, fashioned them artificially, in the expectation that they would serve any purpose whatsoever.

When I handed a stone to Mr. Burchell, as described in the "flyleaf" letter, I made some such remark as "That is just as good as anything you have published." This opinion I still hold, and now repeat. I do not believe that the Sligo "artifacts" are artificial: I do not believe that my stone is artificial: and  $0 = 0$ . This opinion is irrespective of the nature of the fractures on the stones, an irrelevant matter on which I committed myself to no opinion. I wanted nothing more than a tangible expression of my inability to treat the Sligo objects seriously, and this stone happened to come to hand. Almost any other stone on the beach would have served my purpose equally well. Therefore, when Messrs. Moir and Burchell lay stress upon the contrast between their specimens and mine in this respect, they are merely beating the air.

When Mr. Burchell, on receiving the stone from me, asked me if I authorised him to take it to London, I foresaw that it would be submitted to a meticulous examination, and I thought that it would be highly interesting to see what he and his friends would make of it, although it possessed no interest of its own. Naturally, I told him to do what he liked with it. The verdict is now published, and I am not disappointed.

For the negative part of the verdict I was fully prepared. Anything else would have been surprising. There are probably at least five or six different varieties of limestone in the Rosses beds, each with its own texture and its own consequent peculiarities of fracture. Probably there are five or six different kinds of natural forces acting upon stones from these beds, breaking and otherwise shaping them. There is a countless variety of attendant circumstances modifying the application of those forces in individual cases. It was only to be expected that the fractures of my specimen would not in any respect resemble those of Mr. Burchell's. The chances would be overwhelmingly against a fragment, casually picked up, falling into line with any special series carefully selected from the available beach material. The equation  $0 = 0$  says what it means, perhaps yet more impressively, when we select the cyphers from different founts of type.

On the other hand, the positive side of the verdict was a surprise. Here is a stone found on a very rapidly eroding beach, probably not long detached from its parent bed from beneath a cliff at the north end of an island, and therefore for the greater part of the year in shadow, and that in a region which at best does not enjoy an excess of sun: from below high-water mark, and therefore for the greater part of the day under water, and even when not submerged kept cool and moist by the excessive sloppiness of the beach. This stone, we are told, has been fractured by "thermal disruption," about the last agent that any reasonable being would think of in the circumstances. Later in the day I gave Mr. Burchell another fragment, from a different part of the beach. This, it appears, has been fractured by the same improbable force. Are we to conclude that this was a coincidence—a staggering coincidence? Or that all the playthings of the wintry Atlantic waves are broken by "thermal disruption"? Or that the jury is talking nonsense about the fractures upon these stones, whose origin is known, in order that we may estimate aright the theories which they weave about the fractures upon stones whose origins are unknown?

Messrs. Moir and Burchell complain that I have formed and expressed an opinion without having seen the specimens. I claim the same right of judging at a distance which is exercised by Mr. Moir himself, who has given his benediction to the (slightly amateurish) geological observations of his colleague, while admitting that he has never been near the place where those observations were made. To say nothing of the unfavourable judgments

which I have received from correspondents whose opinions I respect, and who have examined the specimens, I proceed on the assumption that Messrs. Moir and Burchell have themselves provided those, to whom the originals are inaccessible, with materials to come to a decision about them. I have a higher opinion of their plates than they themselves seem to have. I presume that the specimens there selected for illustration are the best available, and that as the plates are wash drawings, not photographs, the evidence of human workmanship will at least not be minimised. I take down their book and turn over the plates, and I ask myself if the relics of shore *débris* there illustrated are really worth a journey which will cost me a day's work, two nights' sleep, and a five-pound note; and the plates are quite good enough to help me to reach a negative conclusion. Next, I set beside these plates those in the volume by MM. Boule and Villeneuve on the "Grotte de l'Observatoire" at Monaco, which has recently come into my hands, and which has been quoted (not without temerity) in connection with the Sligo objects. That is enough for me. I close the Sligo book, and I return it to its appointed place on the topmost shelf, where the dust accumulates on curiosities of literature such as treatises to prove that Ireland was Ur of the Chaldees, and that the Round Towers were temples of Apollo.

Since the above was written, I have been obliged to spend a few hours on other business in London, and have been able to repair my omission. I snatched a few moments of free time, and inspected the selection in the British Museum. I saw no reason to change a word of what I had written. On the contrary, the conclusion which I reached was that the specimens were far worse than I had been led to expect.

Unfortunately, it is now too late for Mr. Moir, in his turn, to repair his omission; it is certainly not worth his while to travel to Sligo to see the "rock-shelter," so called, now that it has passed under the hands of his colleague. The latter believed that he was dealing with the only known Palæolithic site in Ireland. In that belief, he has turned over its stones, smashed them up, flung them about, and, in a word, "hogged" the site like any excavating dealer. The belief was a mere delusion; the site is of no real importance, and therefore no serious harm has been done. But these comforting facts do not exonerate Mr. Burchell, nor diminish the value of the episode as an illustration of his ideas about the treatment of ancient sites.

Yours faithfully,

R. A. S. MACALISTER.

Ireland: Archæology. Whelan.  
**Ireland In Pleistocene Times.**

To the Editor of MAN.

SIR,—I recently discussed with a **124**  
 follower of the Irish "opposition" the  
 claims advanced by Mr. J. P. T. Burchell  
 and Mr. J. Reid Moir for the Lower  
 Palæolithic origin of the Rosses Point  
 limestone artifacts. As is usual, he had  
 neither visited the sites nor subjected the  
 series to critical examination, but did not  
 scruple to pronounce servile judgment in  
 the word "impossible." His attitude was  
 laughable in its naïve admission of *parti*  
*pris*, and, as he had already muddled the  
 waters of controversy with much pseudo-  
 scientific journalism on the subject, I took  
 the trouble to invite his reasons. He had  
 seen a map of the Irish terminal moraines.  
 I suggested with some malice that certain  
 students of Mr. Burchell's discovery,  
 including myself, were inclined to assign a  
 date prior to the Irish ice and to correlate  
 these flakings in the grand manner with  
 the Cromer Forest Bed. A little later he  
 telephoned to say that there could be no  
 comparison, since Cromer had not been  
 glaciated! This critic has subsequently  
 published a further diatribe of some fifteen  
 hundred words, no doubt for the guidance  
 of non-archæological readers, in which the  
 opinions of those geologists and litho-  
 classilogists now ranged in support of  
 Mr. Burchell's conclusions are by implica-  
 tion offered for the ridicule of the market-  
 place. I mention this incident as a fair  
 example of the disingenuous propaganda  
 —perhaps inevitable where *anciens régimes*  
 are challenged—with which from unin-  
 formed parochial quarters the public mind,  
 naturally and properly conservative in such  
 matters, is being exploited.

A very different issue is joined by  
 Professor J. Kaye Charlesworth in his reply  
 (MAN, 1928, 84) to some questions which  
 I ventured to put to him in this journal.  
 It is true that not the least important of  
 my enquiries he leaves, unanswered, to his  
 fellow-geologists. One I had thought to be  
 already the concern of the geologists;  
 namely, the discovery of what is evidently  
 a glacial moraine at Hastings above and  
 below the White Chalky Boulder Marl.  
 Moreover, I pointed out in my earlier note  
 that Professor Charlesworth had opened his  
 attack upon the Sligo claims by a positive  
 and reiterated assertion that the flakings in  
 question were nothing more or less than  
 angular beach material, and I drew atten-  
 tion to the fact that this curious *ex cathedra*  
 pronouncement was made without the  
 customary scientific precaution of seeing  
 the specimens under reference. The un-  
 wisdom of rash assumptions and of scorn  
 for established beliefs with which Professor

Charlesworth charges me is surely better  
 demonstrated by his own example. Fortu-  
 nately for those who wish to weigh the  
 evidence apart from partisan influences,  
 Professor Charlesworth's article appears  
 together with one from the dispassionate  
 and subtle pen of l'Abbé Breuil, whose  
 analysis of the Sligo technique provides the  
 best possible compensation for statements  
 unwarranted by observation or experience.  
 By way of additional comment on the  
 notorious "storm beach theory," it may  
 interest Professor Charlesworth to know  
 that a specimen collected by myself during  
 last summer from the shelter site has, after  
 prolonged study, been declared by a  
 prominent archæological opponent of the  
 claim to be due to the average  
 effects of ice pressure and, while simulating  
 prehistoric work as brass resembles gold,  
 to constitute in both respects an important  
 test piece! One notices with appreciation  
 and some relief that, in the letter upon  
 which Professor Charlesworth relies for the  
 demolition of Mr. Burchell's geological con-  
 tentions, Professors Jones and Boswell  
 have expressly refrained from giving any  
 opinion on the question of the human or  
 natural origin of archæological material  
 which they have not seen.

Professor Charlesworth objects to my  
 acceptance of the Penck-Brückner glacial  
 sequence for the British drifts, and after  
 complaining—as I think, unreasonably—  
 that I have misrepresented his views or  
 his expression of them to non-archæological  
 readers, himself introduces the novel  
 suggestion that Mr. Burchell's supporters  
 fail to distinguish between glacial and  
 interglacial conditions in the vicinity of  
 the axes of radiation and those remote from  
 the centres of dispersal. One had thought  
 that by this time most of us might be  
 allowed credit for a nodding acquaintance  
 with current criticism of the Penck scheme  
 and its labels on the one hand, and of the  
 monoglacial theory on the other. Fruitless  
 searches for a Günz phase in Northern and  
 Western France and farther afield have  
 by no means escaped lay attention, and  
 the attribution of the Chillesford and  
 Weybourne Craggs to a diluvial opening  
 phase of the Pleistocene does not, so far  
 as I am aware, disprove the ancient proverb  
 of the rose's name. Does Professor  
 Charlesworth disassociate himself from  
 acceptance of the first general glaciation—  
 without whisper of a Mindel correlation—  
 as the period of maximum ice dispersion in  
 this country? It may well be, and from  
 considerations of typology alone it seems  
 to me more probable, that the Sligo artifacts  
 relate to human inhabitation of North-  
 Western Ireland before the onset of glacial  
 conditions proper. There is, however, a

very definite alternative. My chief concern with Professor Charlesworth's views on an Irish Palæolithic and the purport of my criticism was quite explicitly an attempt to reduce to terms of scientific exactitude his views—as expressed to non-archæological readers—on what succeeded to the primary glaciation. It is somewhat reassuring to be told that he does not contemplate tundra conditions in true inter-glacial times. One had hoped to know also whether he agreed as to the probability that in Ireland, as elsewhere in North-West Europe, the ice of the primary glaciation melted away, with the consequent exposure of a wide land surface, much of which may now, in the form of striated pavements, be buried in succeeding drifts. The geological evidence, as well as that of fauna and flora, points to something more than an oscillation of the ice margin in Southern and Eastern England, and it is extremely difficult to believe in or to find positive supporting evidence for the view that the great climatic amelioration which followed the maximum extension of the ice-sheets was unproductive throughout the British Isles of "true" interglacial conditions. In my own belief, it is not *prima facie* improbable that towards the close of this period a race which had inherited certain of the Early Mousterian culture forms occupied the limestone cliff shelters now discernible along portions of the Sligo coastline and hinterland. Nothing is more obvious than that implements of interglacial date demand in Sligo a thorough-going deglaciation and the paucity of evidence must admittedly be faced. No one proposes to dictate to Professor Charlesworth the methods by which he finds his particular glacial superstructure. I do propose, however, to bring before the London Society of Antiquaries this autumn, in connection with Mr. Burchell's forthcoming paper, a limestone percussive flake having truncated flake scars which I recently recovered in the Ballyconnell boulder clay section. This specimen was obtained from stream-bedded deposits intercalated between the clays and exhibiting marked contortion from the ice passage to which the upper till is due. The similarity which it bears in form and technique to the material from Rosses Point is too close to be fortuitous, and it has been accepted as indubitably human by those best qualified to judge. What influence the additional evidence now available may have upon scientific opinion remains to be seen, but in passing one may point out the error of Professor Charlesworth's allusion to limestone as a new prehistoric material. It is not the first time that this unaccountable assertion has been made, and it may be hoped that after

l'Abbé Breuil's references this particularly inane method of discounting the Sligo finds will now be dropped.

It has been for so long the basis of Irish glacial theory that no interglacial traces exist that any effort to undermine the negative argument seems sacrilege. None the less, Professor Charlesworth is well aware that explanations of the lack of evidence are obvious to the point of platitude, and that some important evidence survives. It is generally agreed that the major interglacial was succeeded in North-West Europe by a younger diluvial ice age of climatic oscillations between extreme cold and relative mildness and that the glaciations of this period were straddled by Mousterian man. If the Sligo tills should represent local reactions to the glacial phases of this period, strong confirmation is given to Mr. Moir's contention that the Rosses Point implements are of Early Mousterian date. The severity of the concluding ice movements of the Pleistocene is not to be denied, and they must in Scotland and Ireland have obliterated or completely reassorted the deposits of preceding epochs with a thoroughness unknown in regions further removed from the icesheds. In such circumstances the reconciling of the various phenomena must indeed be a thankless task. Some salient facts seem, notwithstanding, to emerge. The Scottish evidence of an interglacial depression indicates clearly enough that, before the advent of the last general *mer de glace*, the North Sea was occupied by a fauna not unlike that of the present. There is ample geological authority for this statement, and it has a vital bearing on the question in point. It is asked where are the interglacial deposits in Scotland, a country whose geology is substantially in debt to Professor Charlesworth. Certainly, the traces of a temperate fauna and flora are fragmentary, as might be expected for the reason I have mentioned. It stands to commonsense and a little reasoning that, in regions subjected to recurrent and enormously severe local glaciation, the memorials least likely to survive are those of temperate conditions. Among the most significant Scottish exceptions is, or rather was, the famous cutting of Cowdon Burn, which disclosed lacustrine deposits of gravel, sand and brick-earth, interspersed with layers of peat and intercalated between two boulder clays. These beds yielded *Bos Primigenius*, *Cervus Megaceros*, and many remains of a varied temperate fauna and flora; including six or more species of *Coleoptera*, *Chalcididae* belonging to four or five species, *Daphnia* in great abundance, four species of moss, and the seeds of twenty-eight kinds of

flowering plants such as *Betula alba*, *Corylus Avellana*, *Salix repens*, *Pinus sylvestris*, etc. Such instances can be multiplied, and cannot be annihilated by a judicious use of the rhetorical question. In Ireland it is, I think, probable that more zealous field work and painstaking analysis of our brick-earth and peat intercalations will ultimately accomplish wonders. It is undeniable that a considerable amount of field work has in the past been so much wasted energy. As an example of this defect, there is Keshcorran, which, as Professor Charlesworth points out, has furnished as yet no hint of temperate inter-glacial fauna. Had he examined, as I have done, the late Mr. Ussher's surface scratchings in those caves, he would have realised that an adequate investigation of their extensive and doubtless pregnant deposits was not begun and could scarcely have been contemplated. It is common knowledge, also, that when excavating a small portion of the late glacial drift, Ussher was obsessed by the idea of unearthing *large* bones and placed a corresponding value on his finds. In fact, it is little short of the mark to say that such results as were achieved in this direction were obtained more by luck than good management. The work seems from report and from the present appearance of the excavation to have been superficial and highly imperfect, and, as I suggested in my former paper, merely proved the existence of limestone caves in Pleistocene times, with late glacial fauna, in the neighbourhood of Mr. Burchell's discovery. Were an effective scientific examination of the Keshcorran cave deposits made, I am confident that more than a hint of inter-glacial occupation would be forthcoming.

Professor Charlesworth places on record his opinion that incontrovertible confirmation of the alleged recent origin of the Rosses Point site is afforded by the letter of Professors Jones and Boswell to *Nature* (2nd June, 1928). As the drift specimens are unassailable from the geological standpoint and the Coney Island cave is immaterial to the issue, this challenge goes to the root of the problem. It is interesting, therefore, to note precisely what these investigators have reported on the matter. They say, "Whatever, therefore, may be the position as regards the human origin of the flakes found beneath the loose blocks, it is improbable that a 'shelter' could have existed there in Palæolithic times." This is in no way as sweeping as the categorical refutation to which we had previously become accustomed. It admits of a second opinion, and that the contrary interpretation to be advanced to the London Society of Antiquaries will find general acceptance I have personally

no misgivings. I cannot anticipate Mr. Burchell in this matter, but in conclusion I would suggest that those interested should await the final report of the discoverer before complying with Professor Charlesworth's appeal in visiting a site which presents few vestiges of its original aspect.

C. BLAKE WHELAN.

#### Anthropology, Physical. Hornblower. Right-handedness.

To the Editor of MAN.

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SIR,—In No. 104 of this month's issue, Dr. Harrower instances the ancient Egyptians as a left-handed race, on the strength of some funerary sculptures and paintings. As to the sculptures, women carrying things on their heads are as likely to steady their burdens with one hand as another, often easing their toil by changing hands. With regard to paintings, the artist was, in ceremonial scenes, bound by conventional rules of symmetry: hieroglyphs can be written facing either way, in wall-paintings they always face the chief point, whether it be at the centre or the end of the wall. Human figures follow the same rule, so that if the chief point, for example a "false door," is in the centre of the decorated space, the figures on each side face towards it; similarly if they hold in one hand an object of importance, such as some kind of sceptre or wand of office, it is represented as conspicuously as may be, that is, in the hand nearest the spectator, which may be the left or right according to the direction in which the figure faces.

Further, the artists were often far from realistic: in wall decoration it is very common to find the hands of a figure represented as both right or both left; and with the feet always in profile; this is the practically universal rule.

A perusal of Wreszinski's "Atlas" will furnish plentiful examples; taking, at random, plates 402 and 420, we find scribes writing with their right hands and workmen at one-handed jobs using their right hands, guitar players pluck the strings with that hand and sportsmen throw boomerangs. A good example of an exception caused by the rule of symmetry is seen in Plate 294, of the time of Amenophis II; there is a large central feature in the shape of a broad pillar, on each side of which are the papyrus boats of two sportsmen, each faces towards the pillar, as do the sterns of the boats, quite symmetrically; the left-hand man uses his right hand to throw the boomerang, while his companion facing him brandishes his fish-spear in his left hand, their weapons being, of course, objects of importance in a scene of sport.

G. D. HORNBLOWER.





*[Photo by J. Palmer Clarke, Cambridge.]*

BARON ANATOLE VON HÜGEL.

## ORIGINAL ARTICLES.

With Plate K.

## Obituary.

Haddon: Maudslay.

**Baron Anatole von Hügel.** By A. C. Haddon, Sc.D., F.R.S. With **126**  
Plate K.

Anatole von Hügel, who was born in Florence on September 29, 1854, was the second son of Charles, Baron von Hügel, who married the daughter of General Farquharson in 1851. Charles von Hügel (1795-1870) had a brilliant career as a soldier, diplomat, and man of science. During the years 1831-1836 he was engaged in extensive and very successful scientific travels in Asia and Australasia, during which he made very large botanical, zoological, and other collections. For his geographical research in Kashmir and elsewhere he was awarded the Patron's Medal of the Royal Geographical Society in 1849. He was a great gardener, his gardens and conservatories being of European fame, and he did more than anyone else to encourage horticulture in Austria. He was learned in several branches of science and was a highly accomplished and charming man. Anatole inherited many of his father's tastes, and from his earliest days associated with travellers of distinction and men of science and culture; thus was his character formed and his future life determined.

Anatole came to England in 1867 and his home since then has been permanently in this country, to which he has been consistently most loyal.

He was sent in 1874 by his doctor to Australia for a voyage there and back in a sailing ship, but for four years he remained in Australasia, and collected natural history specimens in Australia, New Zealand, Fiji, Samoa, and Java. Shortly after reaching Fiji in 1875, and before the proclamation of the group as a Crown Colony, he diverted his attention from ornithology to the natives, as he realised that this might be the last chance of studying them in their original condition. He therefore, travelled into the practically unknown interior of Viti Levu, despite the strong dissuasion of European residents. Considerable excitement prevailed at that time among the natives owing to impending British rule, but he made close friends with his hosts, collected a large number of ethnographical specimens, and wrote voluminous notes on what the natives did and made, and he soon became an acknowledged authority on Fiji. His example stimulated the new Governor, Sir Arthur Gordon (later Lord Stanmore) and his suite to make similar collections. All these collections, with some subsequent choice additions, form an unrivalled display in the Museum of Archaeology and of Ethnology in Cambridge. The appended note, by Dr. Alfred Maudslay, gives a vivid picture of von Hügel in Fiji.

In 1880 he married Eliza Margaret, eldest daughter of William Froude, F.R.S., and niece of Hurrell Froude and the author James Anthony Froude, and those who had the privilege of their friendship realise what a happy married life they had and how much the devotion and loyalty of his wife relieved the strain to which he was subjected.

As the Gordon and Maudslay collections were promised to Cambridge, von Hügel, on account of his special qualifications, was appointed, at the end of 1883, the first curator of the museum of General and Local Archaeology, which consisted of the collections the Cambridge Antiquarian Society had recently presented to the University.

Von Hügel at once, and throughout his life, did all he could to develop the archæological department of the museum, and all sections of it were greatly increased. He acquired a considerable and comprehensive knowledge of British stone and bronze implements, and was greatly interested in Saxon pottery and personal ornaments,



many examples of which were excavated by himself, nor did he neglect medieval and later objects of antiquarian interest.

For many years the collections were lodged in the Museum of Classical Archæology; the exhibition space was cramped, and, as von Hügel worked in a very small room under most unhygienic conditions, there can be little doubt that this seriously injured his health, which was never robust. The examination, registration, and labelling of specimens were thus performed under extremely unsatisfactory conditions.

It soon became apparent that a new museum would have to be built to accommodate the specimens, which were increasing at a rapid rate owing to continual gifts of large and small collections and individual specimens, very many of which were given by von Hügel himself, the Baroness, and other members of his family, as well as by numerous friends of his, for the curator was such a charming beggar for the museum that his friends could not resist his appeals. He thoroughly realised that there was no time to be lost in obtaining ethnographical specimens, as the opportunity for doing so was fast disappearing. Although he paid attention to other regions, naturally his chief concern was with Oceania, of which he had such good knowledge, and the collections in the Museum are a lasting memorial of his acumen.

Then began for von Hügel a very strenuous time. He wrote with his own hand innumerable letters, and, though he hated writing begging-letters, his zeal overcame his repugnance and led to most gratifying results. In the appeal for funds he was helped by prominent members of the University and, as usual, he himself and members of his family made repeated large contributions. Eventually enough money was raised to warrant the erection of a part of the projected museum. Von Hügel devoted great attention, in consultation with the architect, Sir Graham Jackson, to the details of the structure and fittings, which led to substantial improvements. The foundation stone of the first block of the new Museum was laid by Eliza Margaret, Baroness Anatole von Hügel, on May 14, 1910, the Vice-Chancellor and many well-known members of the University being present on the occasion. Two years later the removal took place, but previously there had been months of labour expended in the careful packing of the specimens to prevent any possible damage to them.

The weary work of raising fresh funds for other blocks of the Museum was now renewed, and the unfinished building as it now stands is already too small. In addition there was the labour and worry of installing the specimens in the new building and the endeavour to cope with the registration of new collections and specimens. During all these years von Hügel worked to the limit of his strength and frequently beyond it. As is well known, he continually suffered from ill-health and the worries and anxieties told on his sensitive nature. The welfare of the Museum was never absent from his thoughts, and he lived to see its assured prosperity and its recognition by experts as being of first-rate importance.

In the autumn of 1920 his health quite broke down and in June 1921 he felt obliged to send in his resignation as curator as from December 31, 1921. As health permitted he continued to work in the Museum and had the satisfaction of completing the installation of the Fijian collections. The end came on 15th August, 1928.

The above-mentioned circumstances, combined with a difficulty in expressing himself in writing and a natural diffidence, afford sufficient reasons why he has little published work to his credit, and help to explain why his long-projected and much-looked-for monograph on Fiji has never been finished. It is doubtful whether there is anyone with sufficient knowledge to see through the press a manuscript which is deprived of those illuminating reminiscences and suggestions which an original investigator alone can furnish. It thus comes about that up to the present von Hügel's permanent memorial is the Museum. Those who remember the condition of affairs before and during the closing decade of the last century will know that von Hügel has not laboured in vain.

After being appointed curator, von Hügel was made an honorary M.A. of the University and he then joined Trinity College. In May, 1922, he was given the Sc.D. *honoris causa* for his distinction as an ethnologist and for the great work he had done for the University.

Von Hügel loved his beautiful garden and those who have been privileged to walk round it with him will not forget his loving care of and personal interest in every individual plant, many of which had been brought home during his travels abroad. He and the Baroness were the kindest of hosts and neighbours, and both took an affectionate interest in the affairs of their friends, which was extended to their friends' children and grandchildren.

No account of von Hügel is complete without the recognition that he was a deeply religious man, and that his whole life was permeated by a devout faith and charity towards others. Roman Catholicism in Cambridge owes a very great debt to him, as the consolidation of Roman Catholic forces within the University was largely due to his enthusiasm and help. He was more than a benefactor to his Church, he was an inspiring influence.

A. C. HADDON.

Sir Arthur Gordon had taken over the Government of Fiji in June, 1875. I arrived in September and found the Baron already installed in Government House at Nasova. He had been picked up by young Arthur Gordon in the mountains, half starved from trying to live on native food, and having parted with everything down to the buttons on his clothes, in exchange for native earrings and other small ornaments. I believe he came to Fiji a few months earlier in search of ornithological specimens, but soon abandoned that in his delight in native wares. He lived in the next room to me, only divided off by a reed screen, and would sit up late at nights writing his notes and long letters, some of which I hope have been preserved. I used to get him out of bed with the end of a fish spear through the screen, to go for our morning swim. It always reminded me of the pictures of the "Pied Piper" to see the Baron in a native village with a tail of children hanging on to him and begging the "Baroni" to come back and play with them. And I shall never forget a memorable visit to Bau, the miniature island capital, where the Baron and I went to await the arrival of the Governor's party, which never turned up, delayed by rumours of war. We spent the week with the natives. Thakombau was very genial, and when we drank *kava* with him, hearing that the Baron had been up in the mountains, he put his arm round him and felt his ribs, and, laughing, said he wondered the Kai Tholo had not eaten him. His widowed daughter Audi Quila, who was a great lady and governor of a province, was devoted to the Baron and always called him her son. We wandered about from house to house, always genially welcomed, and found out where the mats were softest, and where were the most attractive damsels to make *kava* for us, and we were entertained with native stories and native songs. The Baron was a favourite everywhere; his sweet temper and childlike simplicity appealed to the native mind and always made him the most delightful companion.

ALFRED P. MAUDSLAY.

Anthropology, Physical.

Young.

**The Problem of the Racial Significance of the Blood Groups.**  
By Matthew Young, M.D. (continued from MAN, 1928, 116).

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The blood groups are known to be very stable under varying environmental conditions. The concordance between the distribution of the groups in Hungarian gypsies as examined by Verzar and Weczeckzy (1922), descendants of some who are believed to have migrated from India in the twelfth century, and the distribution of the blood groups in Indians of the present day as examined by the Hirschfelds, and

its divergence from that of the Hungarians amongst whom they live has been frequently cited as evidence of the persistence of the type of blood grouping from generation to generation in the absence of crossing. It is very doubtful if the results of 1,000 observations can be accepted as in any way representative of the blood grouping in India, in which there appears to be evidence of several racial types. Perhaps too much emphasis has been laid on this particular example of apparent persistence of blood type. Peoples as far apart from one another and as divergent in physical type as the Senegalese and the Sumatrans exhibit identical distributions of the blood groups, whereas the distribution in the former group diverges significantly from that shown in the Melanesians on New Guinea, who are negroid in type and probably derived from the same primitive stock. The fact that the relative distributions of the blood groups are apparently not significantly differentiated in data derived from peoples as divergent in physical characters as Viennese and Japanese, Egyptians and Ainu, Russians and American Negroes, Spanish Jews and Chinese, Australian colonists and Australian aborigines and Swedes and Lapps respectively, as shown in Table I, although in most cases these inferences are based on fairly large numbers of observations, quite comparable in extent at least with those of which blood group records appear frequently at the present time in publications, suggests that the blood group differences may not be so valuable in racial differentiation as is now more or less generally believed. It is doubtless true that resemblances and differences in blood group proportions may give in many cases some indication of racial relationships, but with the available data and in the present state of knowledge regarding the probable origin of the mutations which may have appeared not once only but repeatedly, it is difficult to draw any but the most general inferences regarding possible relationships. So far as can be seen at present, there would appear to be small warrant for the belief or claim that the evidence supplied by blood grouping will provide a complete and satisfactory basis for the classification of racial types and that more importance should be attached to this feature than to other racial characteristics such as head or hair form or skin colour. When more is known regarding the distribution of blood groups in other samples of peoples of definite racial types, of whom no record has yet been obtained, the characteristic may be shown to be of greater value as a criterion of racial differentiation and classification, but the present indications are that blood grouping will not replace but may possibly supplement in some measure the racial criteria at present relied upon.

The criticism may be advanced that, in the present study, I have selected the pairs of unlike race based on anthropological characters which do not show differentiation in their blood grouping and the pairs alike anthropologically which do show differentiation in their blood groups; in other words, laid emphasis upon the "failures" and ignored the "successes." If it be supposed that on general anthropological grounds six races can be distinguished, in order to provide a crucial test of the value of blood group distribution as a racial criterion, two samples of blood distributions from every possible pair of groups of similar race and from pairs of unlike race should be compared. If the percentages of differentiations were significantly the same in the two classes, it might be considered conclusive proof that blood grouping was of little value as a racial criterion. A survey of Table I, in which a considerable number of comparisons of blood group distributions of population groups selected from among the most reliable and most comprehensive data available at once reveals a difficulty. It is apparent that in relatively few instances can the proportions recorded be regarded as representative of a definite "race"—a group subject for a long period to a stable environment and mating with one another freely (Karl Pearson). As already mentioned, there is no unanimity of opinion among anthropologists as to the most suitable classification of peoples into

racial groups. Perhaps there may not be sufficient warrant for the assumption that the classification of races based on physical characters can be regarded as final or conclusive, nor is it certain that the classification based on blood groups should be expected to coincide with the former. It is well known that any racial classification depends wholly on the criteria used and that even the use of alternative groups of two or three characters would provide dissimilar classifications. From the considerable variation shown in the percentage distributions of the blood groups in successive hundreds of the same series of Australian colonists by Tebbutt, it is apparent that little reliance can be placed on percentage distributions based on even 100 observations, although numerous results based on smaller numbers than this are published and conclusions drawn from them. If the attempt be made to obtain more extensive or more adequate records by combining the figures of different observers, the risk is very great that the special features of definite anthropological groups will be obscured, if not lost. For example, the percentage distributions in fully 11,000 Poles recorded by Halber and Mydlarski, and of approximately 9,500 Japanese obtained from the aggregated data of several Japanese observers, including Nakajima, Kawaiishi, Furuhashi, Masubara, Hara, Kishi, etc. (for other names see Furuhashi, T. and Kishi, T., 1926) are as follows :

Population Sample.	Blood Group Percentages.				Total No. of Observations.
	O	A	B	AB	
Poles - - - - -	32.5	37.6	20.9	9.0	11,488
Japanese - - - - -	30.4	38.5	21.5	9.6	9,337

Similarly, fairly large groups of Russians and Chinese give the following figures :

Russians from Moscow (Wagner) -	32.0	38.5	23.0	6.5	2,200
Chinese from Hunan (Li Chi Pan) -	31.3	38.1	20.7	9.9	1,500

To the ordinary observer, the distributions in the Poles and Japanese and the Russians and Chinese respectively may be considered as practically identical and might be assumed to indicate that Poles were of the same racial type as Japanese and Russians as Chinese. Owing to the relatively large numbers on which these percentage distributions are based, however, the differences are greater than might reasonably be expected to arise from random sampling, as may be shown from values of  $\chi^2 = 11.02$  and  $P = 0.012$  in the first instance and  $\chi^2 = 15.40$  and  $P = 0.0015$  in the other. The respective pairs are differentiated in respect of their blood groups, but this conclusion depends on the relatively large numbers of observations. The principal Japanese data have been combined to obtain more adequate numbers, but, by this procedure, differences in the distributions in the several districts of Japan already described and emphasised by Japanese writers have been obscured, and yet it cannot be assumed that these divergencies may not be of some importance. It may be mentioned, further, that there is very grave doubt as to whether the records of certain observers are reliable or truly comparable with others, but to what extent this defect may be due to variations in technique or other causes, it is difficult to offer an opinion.

As it is impossible to decide upon which comparisons of pairs should be included as belonging to groups of similar race in order to calculate percentages of "successes" and "failures" in groups of like and unlike races—a procedure kindly suggested to me by Professor Greenwood—it has been considered sufficient to publish the values of  $\chi^2$  and  $P$  that have been obtained from comparisons of the blood group distributions in many pairs of population samples derived from the recent literature on the subject. These are given in detail in Table I. Many of these are from samples of the same or closely related peoples and the number of instances in which these samples appear differentiated with respect to blood group distribution whereas groups, racially dissimilar by almost every canon hitherto

TABLE 1.

Samples of Peoples Compared.				$\chi^2$	P.	Remarks.
1,000 Viennese (Hoche & Moritsch)	and	500 Japanese in Tokio (Nakajima)	-	1.73	0.63	Undifferentiated.
417 Egyptians (Mooko & Dolbey)	and	205 Ainu (Ninomiya)	- - - -	2.49	0.48	"
500 Swedes (Lindberger)	and	183 Lapps (Schött)	- - - -	1.33	0.73	"
1,176 Australian Colonists (Tebbutt)	and	191 Australian Aborigines—pure bloods (Tebbutt).	-	3.31	0.35	"
500 Englishmen (Hirschfeld & Hirschfeld).	and	191 Australian Aborigines—pure bloods (Tebbutt).	-	5.52	0.14	"
1,000 Russians (H. & H.)	and	500 American Negroes (Snyder)	- -	5.69	0.13	"
500 Spanish Jews (H. & H.)	and	1,000 Chinese (Liang)	- - - -	2.42	0.48	"
435 Manchus from Mukden (Fukumachi, Kiri-hara, Haku).	and	600 Ainu (Ninomiya, Kishi, Nakajima)	-	0.37	> 0.80	"
750 Germans in Berlin (Schiff & Ziegler).	and	500 Turks (H. & H.)	- - - -	1.09	0.78	"
500 Germans in Kiel (Steffan)	and	500 Bulgarians (H. & H.)	- - - -	4.42	0.22	"
400 Ukrainians (Manuila)	and	171 South Koreans from Zennan (Kiri-hara, Kaku).	-	4.53	0.21	"
500 Swedes (Lindberger)	and	502 Alpine Germans in Peterstal, Baden.	-	3.33	0.35	"
500 Swedes (Lindberger)	and	1391 Italians (Mino)	- - - -	2.84	0.41	"
546 Sumatrans (Bais & Verhoef)	and	500 Senegalese (H. & H.)	- - - -	0.41	> 0.80	"
1000 Indians—Natives of India (H. & H.).	and	385 Hungarian Gypsies (Vérzar & Wezczecky).	-	4.51	0.22	"
461 Slovaks in Roumania (Manuila)	and	947 Sardinians (Romansen)	- -	6.38	0.10	"
500 Swedes (Lindberger)	and	533 Swedes (Hesser)	- - - -	2.11	0.55	"
500 Swedes (Lindberger)	and	436 Norwegians (Jerivell)	- - - -	0.88	> 0.80	"
582 Germans in Schleswig-Holstein—fr. small towns (Gundel).	and	345 Germans in Schleswig-Holstein—fr. small towns (Gundel).	-	2.15	0.55	"
500 Bulgarians—Prisoners of War (H. & H.).	and	372 Bulgarians in Roumania (Manuila)	-	5.98	0.11	"
500 Negroes in U.S.A. (Snyder)	and	250 Bantu Negroes (Snyder)	- -	1.15	0.77	"
1176 Australian Colonists (Tebbutt)	and	500 Englishmen (H. & H.)	- - - -	6.65	0.09	"
316 American Indians var. tribes (Haskell).	and	457 Navajo Amer. Indians—full bloods	-	4.61	0.21	"
112 Middle Koreans fr. Chukoku (Kiri-hara).	and	184 Middle Koreans fr. Phenyang (Fukamachi).	-	6.72	0.08	"
787 Middle Koreans (Kiri-hara, Fukamachi).	and	354 North Koreans (Kiri-hara)	- -	7.80	0.05	"
783 Hungarians in Debreczen (V. & W.).	and	648 Hungarians in Tetish-Transylvania (Manuila & Popoviciu).	-	5.33	0.15	"
476 German Colonists in Hungary (V. & W.).	and	348 Germans fr. Heidelberg (von Dungen & Hirschfeld).	-	5.34	0.15	"
1000 Germans fr. Leipzig (Sucker)	and	300 Saxons in Roumania (Manuila)	-	11.60	0.0091	Differentiated.*
1000 Germans fr. Leipzig (Sucker)	and	1100 Germans fr. Köln (Wiechmann & Paal).	-	47.23	0.000000	"
1740 Germans fr. Berlin (Schiff)	and	1100 Germans fr. Köln (W. & P.)	-	36.08	0.000001	"
1000 Germans fr. Leipzig (Sucker)	and	1679 Germans fr. Schleswig-Holstein (Schütz & Wohlisch).	-	50.01	0.000000	"
1006 Germans fr. Frankfort (Schneider).	and	1100 Germans fr. Köln (W. & P.)	-	10.72	0.0136	"
1000 Germans fr. Leipzig (Sucker)	and	348 Germans fr. Heidelberg (von Dungen & Hirschfeld).	-	7.95	0.047	"
512 Danes in Denmark (Johannsen).	and	800 Icelanders (Jonsson)	- - - -	20.18	0.000158	"
500 Serbians (H. & H.)	and	461 Slovaks—School children in Roumania (Manuila).	-	14.90	0.0018	"
947 Sardinians (Romansen)	and	559 Italians (Rizzati)	- - - -	42.27	0.000000	"
947 Sardinians (Romansen)	and	150 Maltese (Snyder)	- - - -	12.27	0.0066	"
2500 German Swiss in Zürich (Clairmont).	and	543 Swiss (Plüss)	- - - -	10.98	0.0017	"
383 Westphalians—children of parents from western districts.	and	321 Westphalians—children of parents fr. eastern districts (Klein & Ostoff).	-	44.07	0.000000	"
225 Scots (Alexander)	and	403 Englishmen (H. & H.)	- - - -	18.21	0.000405	"
403 Englishmen (H. & H.)	and	218 Englishmen in U.S.A. (Buchanan & Higley).	-	18.62	0.000336	"
204 Filipinos (Cabrera & Wade)	and	183 Filipinos (Pascual)	- - - -	27.16	0.000006	"
183 Lapps (Schött)	and	199 Swedish Lapps (Reitz)	- - - -	11.15	0.0111	"

\* The peoples compared are only considered to be significantly differentiated in their blood group distribution<sup>8</sup> when  $P$  falls below a value of 0.05, or 1 in 20.

TABLE 1—continued.

Samples of Peoples Compared.				$\chi^2$	P.	Remarks.
510	Russians fr. Kasan (Wagner)	and	808 Russians from Charkow - -	9.46	0.024	Differentiated.*
363	Koreans fr. Seoul (Fukamachi)	and	502 North Koreans (Kiri-hara) - -	13.42	0.0039	"
1500	Chinese in Hunan (Li-Chi-Pau)	and	592 Chinese in S. China from - -	44.35	0.000000	"
1000	Chinese (Liu & Wang)	and	592 Chinese in - -	20.19	0.000158	"
1000	Russians (Hirschfeld & Hirschfeld)	and	400 Ukrainians (Little Russian school children in Roumania) (Manuila)	102.68	0.000000	"
238	Senegal Negroes (Hirschfeld & H.)	and	252 Bantu Negroes - -	9.38	0.025215	"
476	Germans from Leipzig (Sucker)	and	1000 Germans from Leipzig (Sucker) - -	16.97	0.000707	"
1293	South Chinese fr. Hunan	and	1000 North Chinese fr. Peking - -	75.40	0.000000	"
100	Australian Colonists (Tebbutt)	and	2nd 100 Australian Colonists fr. same series (Tebbutt)	11.44	0.009	"
1000	Germans in Leipzig (Sucker)	and	1679 Germans in Schleswig-Holstein (Schütz & Wohlich)	50.1	0.000000	"
1391	Italians (Mino)	and	500 Italians (H. & H.) - -	27.9	0.000004	"
5000	Americans (Culpepper)	and	1600 Americans (Moss) - -	101.30	0.000000	"
5000	Americans (Culpepper)	and	1000 Americans (Snyder) - -	27.84	0.000002	"
1172	Hungarians (von Jeney)	and	688 Hungarians (Manuila) - -	40.2	0.000000	"
2372	Roumanians fr. mountains	and	1272 Roumanians fr. valleys (Popoviciu)	15.2	0.0018	"
213	Armenians (Parr)	and	380 Armenians (Kossowitsch) - -	18.5	0.00036	"
653	Armenians (Altounyan)	and	380 Armenians (Kossowitsch) - -	16.5	0.0009	"
1161	Japanese fr. Nagoya (Kawaishi & Furuhata)	and	1002 Japanese fr. Hida (Kawaishi & Furuhata).	75.4	0.000000	"
1786	Japanese fr. Niigata (Miyaji)	and	1002 Japanese fr. Hida (Kawaishi & Furuhata).	34.9	0.000001	"
205	Ainu (Ninomiya)	and	196 Ainu (Nakajima) - -	26.9	0.000006	"
501	Sanal Moros-Philippines (Grove)	and	442 Sulu Moros Philippines (Grove) - -	44.7	0.000000	"
933	Arabs (Altounyan)	and	500 Arabs (H. & H.) - -	9.0	0.029	"
500	Senegalese (H. & H.)	and	500 American Negroes (Snyder) - -	12.3	0.006	"
546	Sumatrans in Padang (Bals & Verhoeft)	and	753 Melanesians in New Guinea (Heydon & Murphy).	31.8	0.000001	"
500	Senegalese (H. & H.)	and	753 Melanesians (H. & H.) - -	34.5	0.000000	"
543	Swiss (Plüss)	and	1679 Germans fr. Schleswig-Holstein (Schütz & Wohlich).	10.8	0.012	"
500	Swedes (Lindberger)	and	500 Italians (H. & H.) - -	22.9	0.00004	"
500	Serbians (H. & H.)	and	500 Italians (H. & H.) - -	10.2	0.0045	"
500	Serbians (H. & H.)	and	1391 Italians (Mino) - -	24.4	0.00002	"
500	Serbians (H. & H.)	and	500 Swedes (Lindberger) - -	12.5	0.0059	"
1679	Germans in Schleswig-Holstein (Schütz & Wohlich)	and	500 Swedes (Lindberger) - -	22.2	0.00006	"
11488	Poles (Halber & Mydlarski)	and	9337 Japanese—Aggregate of data - -	11.02	0.012	"
500	Arabs (H. & H.)	and	5000 Americans (Culpepper) - -	10.51	0.0151	"
1500	Hungarians (Vérzar & Wecezczyk).	and	9337 Japanese - -	13.39	0.0037	"
2200	Russians fr. Moscow (Wagner)	and	1500 Chinese (Li-Chi-Pau) - -	15.40	0.0015	"
5000	Americans (Culpepper)	and	500 American Negroes - -	18.88	0.003	"
818	Polish Jews (Halber & Mydlarski).	and	230 German Jews (Schiff & Ziegler) - -	10.34	0.0165	"
818	Polish Jews (H. & H.)	and	211 Roumanian Jews (Manuila) - -	12.40	0.0063	"
818	Polish Jews (H. & H.)	and	500 Spanish Jews (H. & H.) - -	18.25	0.0004	"
230	German Jews (Schiff & Ziegler)	and	211 Roumanian Jews—School children in Roumanian Manuila.	25.32	0.000014	"
230	German Jews (Schiff & Ziegler)	and	500 Spanish Jews (H. & H.) - -	14.07	0.0028	"
500	Spanish Jews—in Monastir (H. & H.)	and	211 Roumanian Jews (Manuila) - -	26.23	0.000009	"

\* The peoples compared are only considered to be significantly differentiated in their blood group distributions when  $P$  falls below a value of 0.05 or 1 in 20.

recognised, apparently agree in blood grouping within the limits of random sampling, should be sufficient to convince the most ardent supporters of blood grouping as a criterion of racial type or racial relationship that it may not be of such importance in this sphere as they anticipate.

I am indebted to Professor Elliot Smith for the suggestion that a study of the racial aspect of blood grouping might prove of value.

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MATTHEW YOUNG.

## Africa, East: Religion.

Beresford-Stooke.

**Akamba Ceremonies connected with Dreams.** By G. Beresford-Stooke.

128

I have recently been making enquiries amongst Akamba of the Machakos Reserve as to whether they attach any meaning to dreams. It appears that they do not, and that the M'kamba does not ordinarily regard dreams seriously.

Dreams are, however, divided by them into two categories, viz., good and bad. If a man dreams a bad dream, as of the death of a relative, or the burning of his house, he will, the next morning, take a small half-gourd of water outside his hut and drop into it a piece of smouldering ember, at the same time praying to *aimu*\* not to let the dream become a reality.

If, on the other hand, he dreams a good dream, as of riches, many cattle and goats, or flourishing crops, he will fill his half-gourd with milk and water, and,

\* Ancestral spirits.

squatting outside his hut, will spill it very slowly on the ground, at the same time praying to the spirits of any near relatives who have died, with the words: "Drink this milk and water and send me the good things you showed me last night." If the dream is of a wife being pregnant, he will make the added request that the child be a son.

G. BERESFORD-STOOKE.

#### Africa, East: Religion.

Beresford-Stooke.

**Ceremonies Designed to Influence the Fertility of Women.** By **129**  
G. Beresford-Stooke.

The following notes concerning ceremonies at present in use among Akamba of Machakos District may be of interest.

In the first place, these ceremonies, while comparatively common, vary considerably in detail. Each medicine man has learnt the ritual employed by his father before him. A medicine man who specialises in this sort of work rarely deals in any other medicine.

The main constituents which seem to be employed universally are the latex of the wild fig tree and the partially digested contents of the bowels of a sacrificial goat. The ceremony usually commences with the smearing of the woman's navel and loins with fig tree latex.

Then a goat is killed and its intestines are removed. The contents of the intestines, or a portion thereof, are placed in a gourd and mixed with juice extracted from the root of a bush or herb. Different medicine men use different herbs. Some medicine men also add a small sample of the woman's menstrual discharge. The constituents are well mixed together and a small smear is made on the woman's navel.

A small twig is dipped into the mixture, wrapped in a piece of cloth and tied up with string. This is used as an amulet, and after being ceremonially passed thrice round the woman's waist is finally fastened by means of a string round the waist in such a way that the amulet rests approximately over the womb.

The ceremony can only be performed when the girl is actually menstruating.

Some medicine men anoint the woman with the blood of the goat which has been sacrificed, and one method of doing this is by placing the carcase, after the intestines have been removed, over the woman's head.

One condition which is said to destroy completely any prospect of success is when a newly married couple have their first sexual connection when the wife is menstruating. If the woman then fails to bear child, no medicine man can do anything for her.

Otherwise the treatment is said to be infallible.

G. BERESFORD-STOOKE.

#### Australia: Technology.

Russell.

**Some Parrot-beak-like and Curved Stone implements from the** **130**  
**Mid-Murray Region, Australia.** By G. E. Archer Russell.

During the past eight years I have been privileged to conduct periodic explorations of the old camp-sites of the now almost extinct Mid-Murray aborigines. With Renmark as a working centre, my investigations have extended from Lake Barmera (until recently known as Lake Bonney) in the west to the junction of the Darling River with the Murray at Wentworth in the east, embracing, of course, the Rufus River-Lake Victoria country. Particularly exhaustive have been my researches into that part of this region wherein lie the Ral Ral Creek and the Lakes Woolpoolool, Merritee and Coombool.



Here, in a land once populous, but now bereft of native people, many stone implements have been unearthed; and in this paper I will attempt to describe certain of these implements of curved and parrot-beak-like types, the drawings accompanying this paper being intended to explain them. The six implements depicted, with some others of somewhat like natures, were all found by me at different times and places in the Ral Ral Creek country previously mentioned. There being no local natives now living in this region (certainly none still living the life of their forbears and whose testimony would be worth having), I have not been able to gather first-hand evidence regarding the implements, but I think they may safely be classed as belonging to the stone culture of the Mid-Murray aboriginal existent at the time of the white man's coming.

The six implements under review are all flake tools and executed in flint, that is to say, formed on flakes struck from flint cores. The excellent quality of the flinty material locally available to these people elevated the making of certain



FIG. 1.

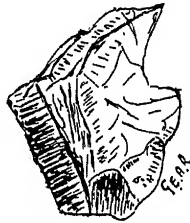


FIG. 2.



FIG. 3.



FIG. 4.



FIG. 5.

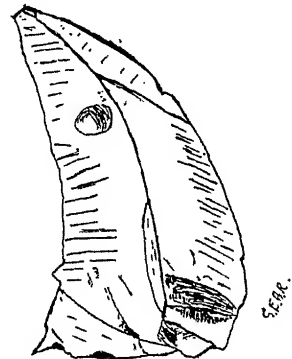


FIG. 6.

stone implements, particularly the smaller forms, almost into a fine art. Relatively small, all of them, these curved and parrot-beak-like tools show workmanship—some more, some less—of no mean order, and were probably the more skilled handiwork of the master craftsmen of the tribe. *Each drawing is made to actual size.*

*Fig. 1.*—This specimen, which has been executed in a somewhat lustrous, reddish-brown flint, is a work of art as well as an implement of utility. It is well-shaped, its upper surface is well trimmed, and it shows along its convex edge secondary working of a high order. A notch shows on the concave edge just below the faceted point; this has been formed not by chipping out a notch, but by so shaping the implement as to leave a projection. This implement, it is interesting to note, is strikingly analogous with a certain so-called parrot-beak graver of Magdalenian times; indeed, if placed in a collection of implements of that

time, it might easily be mistaken for one of them. I submit, however, that the implement under review is not a graver but a borer, used for boring holes in skins, etc., the purpose of the projection just below the point being to militate against forming too large a hole in the material.

*Fig. 2.*—This implement is, in my opinion, a cruder form of the same type as shown in *Fig. 1*, with this difference that there is in *Fig. 2* a steeply inclined back edge, obviously used for delicate scraping or planing. This tool, as may be seen, also has a notch, formed apparently in the same manner as in *Fig. 1*.

*Fig. 3.*—There is an analogy in this type with the Chatelperron points of Middle Aurignacian industry. Another form, obviously of the same type, but somewhat smaller, has the curve reversed. These implements are not notched, and are probably etching tools. Each implement, particularly that shown in *Fig. 3*, shows technique of no mean order.

*Figs. 4 and 5.*—Cruder types, both in design and workmanship. *Fig. 4* is notched by a natural fault in the stone, and is probably a borer. *Fig. 5* is not notched, and is obviously an etching tool.

*Fig. 6.*—There is a resemblance in this type to a certain form among the primitive curved points from the Abri Audit, Dordogne, with this difference—that there is no retouch along the edge as in those of Aurignacian times. I submit that the Australian implement is primarily a point for etching, with two edges for cutting. The small cavity in the implement is a natural fault in the stone.

G. E. ARCHER RUSSELL.

#### Africa, West: Cicatrisation.

Rowe.

**Abdominal Cicatrisations of the Munshi Tribe, Nigeria.** By Capt. 131  
Chas. F. Rowe, communicated by F. J. Richards, M.A., I.C.S.

Extract from a letter dated March 31, 1928:

“ All I know about the ‘stomachic decorations’ of my Munshi tribe (female sex) is that they are apparently of ornamental significance only, as I believe “ is the flattening of the nose of Esquimaux children, but a matter of considerable “ pride to the adult when grown up and the initial pain forgotten.

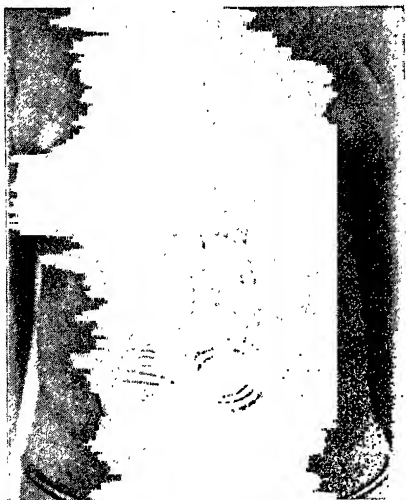


FIG. 1.



FIG. 2.

“ The skin is cut and raised (turned over) and some substance inserted which “ causes a cicatrice about the thickness of a slate pencil on the otherwise rotundity “ and smoothness of the abdomen.

"The operation is performed at an early age and is a 'bloody' sight, and, "from the wails of pain I have heard when seeing it done, is a fairly painful one, "I imagine, if good results are to be obtained (Our artists would term it 'a bold "line"!).

"The 'decoration' is common to the whole tribe, the location of which is "roughly in that area of Nigeria bounded on N. by the Benue River, E. by the "Donga River, W. by 8° 10' E., S. by 6° 20' to 7° 10' N.

"The 'tribe' (Munshi or Tivi) are undoubtedly of Bantu origin."

CHAS. F. ROWE.

## REVIEWS.

Prehistory.

Dawson.

*The Age of the Gods, a Study of the  
Origins of Culture in prehistoric  
Europe and the Ancient East.* By Christo-  
pher Dawson. Demy 8vo. Pp. xx, 446,  
with 4 plates and 10 maps. London:  
Murray. 1928. Price 18s. net.

The notable increase of popular interest in problems of early civilisation, and in the continuity between primitive and modern cultures, has stimulated the production of comprehensive surveys of this field of knowledge. And the very rapid accumulation of fresh data for some of the periods and regions, which were formerly most obscure, has made such surveys both easier to construct, and all the more necessary, if the main outlines of the picture are to be duly emphasised, and not overlaid by the mass of significant and often spectacular details. Of such surveys, often, the best that can be said is that at the moment of publication they are up-to-date; but even this sometimes means that their authors are a little fascinated themselves by the "latest discoveries," and exaggerate their importance. Only occasionally has a fresh writer the courage, and the command of his materials, to reconstruct any considerable part of the general setting, and, as some few painters have done, sit down before a familiar scene and sketch it, literally, from a fresh point of view.

This Mr. Dawson has attempted, and with considerable success. His preface, and the valuable, because carefully selected, bibliography, confess the extent of his indebtedness to recent predecessors; and it is often clear from his presentation which of these have chiefly impressed him. Such eclecticism is unavoidable in so wide a subject, and it would be too much to expect a man to have always something quite of his own to say. What deserves recognition is the large number of points, large and small, which are really observations, and constructive criticism.

The title of the book defines its scope and outlook. Few writers of "Universal History" are equally successful in depicting

what is commonly called the "Ancient East" and the "classical" civilisation of Greece and Rome; and the reason for this is clear. The place of mankind in the realm of nature is different in these two phases of history; and this difference expresses itself most clearly in the rôle which each group of peoples assigned to their gods, and thereby conceived that their own gods assigned to them, in the general order. Down to a certain point, in the growth of experience, or in the diffusion of an "archaic" culture (to which Mr. Dawson is inclined to assign some importance), man is in turn the plaything, the victim, or the favourite, of a "power not himself"—to borrow Matthew Arnold's phrase—more commonly of a loose crowd of such powers: he is in tutelage, a "servant of the Lord," a "child of God"; at highest "under Divine guidance" of One (or Many) "whose ways are past finding out." Beyond that point—and it is here that Greek culture marks the crisis—man conceives himself as responsible for his own doings. Gods there are for him still, powerful, moral, even civilised as man himself is. They can, and do, offer him help, put obstacles in his way, bring home to him the consequences of his deeds. But it is he, not they, to whom ultimate responsibility belongs. Man has become self-determined, grown-up. And the reason for this adulthood is the "steadily growing vision of Reality" (p. xix) revealed as Reason finds scope; that "creative power" which is ever organising the raw material "of life and sensible experience into the "ordered cosmos of an intelligible world," with consequent "possibility of Man's "fruitful co-operation with the powers of "Nature."

With this subsequent phase—a "new vision of Reality" as he calls it—Mr. Dawson does not concern himself directly in this book, though he gives a few hints as to his conception of it. Here he is dealing with its predecessors. A preliminary chapter on the Origin of Mankind, and the Palæolithic phases, leads to the discussion of the mental outlook, and

especially the religious notions, associated with similar phases of culture in modern times. Among these the belief in "Animal guardian Spirits" is presented as fundamental in the "Religion of the Hunter," and the geographical distribution of this type of observance suggests that it may be as ancient as the material craftsmanship and mode of life with which it is actually found correlated later. But "as soon as a people becomes sedentary the vital emphasis is transferred from the life of the hunter to that of the cultivator of the soil, and the earth takes the place of the wild things as the great cosmic mystery. . . ." At this point we miss any reference to *Peasants and Potters*, though its predecessor is duly noted on p. 389, and the argument here follows similar lines. The significance of what is generically the "Peasant Culture," as the fundamental factor in European societies, is well brought out, and its "extraordinary stability and persistence." What Mr. Marvin has called the *Living Past*, is thus protracted far beyond the "great inventions" which we associate with Babylonia and Egypt, and also brought back into our own midst.

Among the "Peasant Cultures" of peninsular Europe, that of the Ukraine, with its painted pottery, challenges comparison with the "painted pottery cultures" of Susa and other sites in Western Asia, and—as we now must realise—far to the north-eastward also. Within this "painted ware" regime, higher civilisation emerges with the use of inscribed seals, the double utility of which, as badge and as amulet, is well brought out (p. 74), and rightly associated in its geographical distribution with that of pictography. A third correlation, with the "city-state," is suggestive; but what does Mr. Dawson include under this term? Can we be sure that this or that ancient aggregation of dwellings and temples was not merely what Greek observers knew as a *komopolis* and distinguished so clearly from their own *polis*; to which it would probably be convenient if the rendering "city-state" were restricted? Greek geographers also distinguished clearly the *hierapolis*, of which Pessinus and Jerusalem are instances; and the great Sumerian cities fall into that class. But they too are "city-states" for Mr. Dawson. And, further—with its seal-stones, and script, and "palace" sanctuaries, and glimpses of a priest king, the sea-lord who "walked with God"—was Cnossus, too, a "city state"? It is a difficult point of terminology, and all the more perplexing as we come to realise how closely the geographical distribution of the primary *polis*-com-

munities of the Greeks coheres with that of the latest Minoan culture. This is only one example of the minor challenges to current notions which Mr. Dawson lets fly as he goes. Others, in the same chapter, are his rehabilitation of the Dravidians "who entered India from the north-west in prehistoric times, bringing with them a higher culture of the Sumerian type," and whom he thinks may be recognised "in the belt of broad-headed population which extends down W. . . . as far as Coorg"; and his derivation of the widespread "Painted Ware Cultures" from a single people "who inhabited the highlands of Western Central Asia, possibly in the region of Seistan," with a notable parallel, geographically, in the spread of Sassanian culture, long after, and—we may add—in an actual phase of aridity like that which Mr. Dawson thinks may have redistributed the "painted ware cultures."

A separate chapter deals with the cults which are correlated with the "Peasant Culture," and especially the worship of the "Great Mother," a great civilising force, whatever its orgiastic extremes.

On the special institutions, and contributions to civilisation, of the Sumerian theocracies, Mr. Dawson traverses familiar ground and has less to offer; his discussion, on the other hand, of the peculiarities of Egyptian culture leads to some interesting points. He is inclined to accept Scharff's very low dates for early Egyptian history, on the ground of the great political importance of the Third Dynasty in other respects besides calendar-making; for it would seem to have been at this stage that the theocratic regime took full shape, "one of the most perfect examples of state socialism in history," intimately adapted to the "natural conditions of human life in the Nile Valley," but presupposing "ages of communal co-ordinated effort" for its reclamation. The older animal-gods receded before the "worship of the Dead King and his celestial father and prototype the Sun God," and the preoccupation of all Egyptians with forethought for the life after death transformed their whole civilisation. "Henceforward Egypt always felt herself to be a land apart; she had found a religious idea that consecrated her geographical isolation and her ancient tradition." But in the confusion, and probably the foreign conquests, which befell Egypt from the Seventh Dynasty to the Eleventh, this "great unitary state" was transformed into "the first example in history of a feudal society."

This leads to instructive comparison between the transformed—but never wholly

superseded—"great state" in Egypt, with the theocracies of the "Sun of Heaven" in China, the "Children of the Sun" in Japan, and the Inca regime in Peru; but the question whether their similarities result from "one of the most remarkable examples of convergent evolution in history," or from "diffusion" by "megalith-builders," Mr. Dawson is content to leave open.

Applying similar analysis to the civilisation of peninsular Europe, Mr. Dawson presents, as primary and distinct components, the "Peasant Culture" already examined in chapters iii and v, and that "East-Mediterranean" culture of which Minoan Crete is the fine-flower; itself composite, with an old neolithic substratum akin to that of Asia Minor, Danubian elements superimposed on one side, and Egyptian on the other. The distinct, though cognate, culture of Troy, and also of the Cyclades, shows how that of Crete "was not an isolated phenomenon, but formed part of a wider movement of trade and maritime commerce, which embraced the whole Eastern Mediterranean." Mr. Dawson would even go further, on the strength of "signs of Cycladic influence" in the Don and Donetz region—not very well established as yet—and of East-Mediterranean elements in the first metal-using culture of the Western Basin. A distinct driving force he finds, following Peake and others, in a "broad-headed minority of Anatolian extraction," to whom he attributes the Asiatic place-names (ending in *-ndos*, *-ssos* and the like) which are widespread throughout the region.

Before dealing with the disturbances caused by the inroads of northern peoples into Mediterranean lands, occasion is taken to discuss the course of events in the Western Mediterranean and on the Atlantic seaboard of Europe, and the origin of "megalith-building" culture. Mr. Dawson is impressed—as we all are—by the peculiar distribution of "megalithic" structures, and deals thoughtfully with current statements about that of associated customs; but he notes also the lack of positive traces of Egyptian intercourse, and inclines to attribute the spread of "megalithic" culture to "a movement of maritime colonisation like that of the ancient Phœnicians or the modern Portuguese," stimulated by a demand for valuable metals. He notes, however, that megalithic tombs are "entirely lacking in metals such as copper and gold," and has an ingenious explanation of this. The focus of this enterprise he is inclined to place in the Maltese Islands, on the double ground of their geographical position

between Eastern and Western Mediterranean, and the peculiar elaboration of their monuments; laying also some stress on their traces of contact with the "Painted Ware Culture" of South-Eastern Europe. But he reserves a claim for some Arabian people, while admitting that "there is at present no satisfactory evidence for the existence of megalithic monuments in the Arabian Peninsula." The following chapter (X) on the sequel to "megalithic" culture in Western Europe leads up to a discussion of what he thinks may be kindred elements in the culture of the Western Celts.

Some confusion may arise if successive writers use the phrase "Archaic Civilisation" in different senses. It is hard enough to make out what it originally meant to Mr. Perry: for Mr. Dawson it means the cultures from China to Portugal and Ireland and from Palæolithic to Bronze-Age "described in the preceding chapters" (I-X). But whereas for Mr. Perry it is still actively propagating itself, in the heart of Borneo for example, for Mr. Dawson the question is "why the creative power of the Archaic civilisation should have deserted it when it was still almost in its prime" in the third millennium B.C. His answer is twofold: "the rigidity which seems to characterise a form of culture that has attained a complete equilibrium with its environment"; and "the rise of a new type of warlike society." The latter consideration leads to a careful discussion of Mr. Perry's paradox "that war is a comparatively late development in the history of humanity," and the conclusion that people are "warlike" or not, according to their regional antecedents, and especially as their surroundings are stable or liable to change. This in turn challenges enquiry into the origin and specific social structure of nomadic pastoral societies, the characteristic "religion of the shepherd" with its skyward interests and patriarchal All-father. A novel point is that as nomad pastoral folk in their home surroundings are ill-equipped for aggressive war, it was probably from sedentary and more civilised people that they acquired metal weapons, and "learned the possibilities of organised war," as its victims in the first encounters. The "Archaic civilisation" was not quite so pacifist as it has been painted, and Mr. Dawson contemplates "invasions" of Egypt, after the Sixth Dynasty, which were rather more than a picnic.

The other cause of collapse, Mr. Dawson finds in that unqualified subjection of the individual to the god-king, which is the perfect work of theocracy: for when the god-king defaulted, or was "taken away

"by men of naught," there was impotent anarchy, "pessimism and moral confusion." This is what makes so modern the *Admonitions of an Egyptian Sage*, class-war supervening on Armageddon. And the remedy was in words, not faith: "I have set up my statue, not that ye should worship it, but that ye should fight for it." That war-cry of the Twelfth Dynasty echoes down to the Eighteenth and the Twenty-sixth; to Augustus and Mussolini.

With that entry of the nomad pastorals on the stage of history, of which the appearance of the horse in the cultures of the Near East, and of Europe too, is the widespread symptom, all the main characters in the great drama have been introduced. In Western Asia, where older regimes had advanced further in organisation, the sequel is comparatively clear and simple. But in Europe "Warriors" and "Peasants" interacted more obscurely and variously, and Mr. Dawson's rendering of the process (chapters XII-XIV) must be read in the light of such earlier interpretations as Professor Gordon Childe's *Dawn of European Civilisation* and *The Aryans*. In Asia Minor, where East and West, in the full sense, first met, the position is obscure still; but good use has been made in Chapter XIII of the new data from Hittite archives; and in Chapter XV, of the Homeric evidence for the course of events in the Aegean during the decomposition of the Minoan regime. It is, however, surely, only in a very qualified sense that "the Greek city was the heir of the immemorial tradition of the Archaic culture of Western Asia" (p. 360). Even to regard it as sprung from "the marriage of the Oriental sacred city with the Indo-European warrior-tribe" (p. 361) cuts the Gordian knot with a speculative hatchet. Some day, perhaps, Mr. Dawson will defend his metaphor in greater detail than the scale of his present essay allows.

Still more in need of expansion is his final chapter on the collateral readjustments in Italy. Here an ingenious and very bold solution of the Etruscan riddle is central, which might have been different if Randall MacIver's *Iron Age in Italy* had been available as well as his *Villanovans and Etruscans*.

From what has been selected for comment here, it will be seen what are the merits, and also certain weak points, in an unusually fresh and very suggestive book. In his preface, Mr. Dawson modestly says that his "whole work is entirely dependent upon" the labours of specialists in many fields. But no one in any of those fields will read it without recognising that the picture is more than the paint.

J. L. MYRES.

Switzerland: Archæology. Tschumi.

*Urgeschichte der Schweiz*. By Otto Tschumi. Pp. 1-192 x 20 plates. 133  
Frauenfeld: Huber & Co. 1926.

A new account of Swiss archæology was certainly needed, since no comprehensive work has been published for 25 years. Tschumi's little sketch thus fills a real want. It makes no pretence of resuming in detail the material so well described by Meierli, but aims rather at illustrating the present position of our knowledge by an account of some recently excavated sites. These might have been better chosen—the work on Lake Neuchâtel is scarcely mentioned, while six pages is disproportionately much for the Grachwil vase—and no clear picture of the relations of cultural groups emerges. The last chapter on prehistory in the light of local folk-tales and folk customs is, on the other hand, a most useful addition to the usual archæological treatment.

V. G. C.

## PROCEEDINGS OF SOCIETIES.

### British

### Association.

### Section H.

### (Anthropology).

*Proceedings of Section H (Anthropology) of the British Association for the Advancement of Science, Glasgow Meeting, September 5-12, 1928.* 134

The Section met in the Department of Anatomy of the University of Glasgow, under the Presidency of SIR GEORGE MACDONALD. His Presidential Address, admirable both in matter and in manner of delivery, opened with a survey of the history of archæological studies in Scotland, which began with the foundation of the Society of Antiquaries and what is now the National Museum in 1780, and then dealt with the more prominent and characteristic

features in Scottish prehistoric archæology, the number of the remains in all, greater than was commonly realised, pointing to a relatively dense population in what were now sparsely inhabited areas.

Scottish archæology and culture naturally figured prominently in the programme. PROFESSOR T. H. BRYCE opened the proceedings by initiating a discussion with a series of maps demonstrating the distribution of relics of the Stone and Bronze Ages in Scotland, which raised the question how far such maps were valid evidence for the sources and routes of culture movements. The paper fully attained its object of provoking discussion. PROFESSOR BRYCE, in a further paper, raised the question of the artificial or natural origin

of certain so-called cultivation terraces in Peeblesshire, which some Members of the Section had been given an opportunity of visiting on a Sectional excursion to Dunsyre and Romanno. PROFESSOR BRYCE also described his excavation of a so-called monastic settlement at Eileach an Naoimh. PROFESSOR ALEXANDER LOW described remains unearthed in five Long Cist burials in Kincardineshire, three being males and two females, of low stature (m., 5 feet 6 inches; f., 4 feet 11½ inches), with rather broad heads and square shallow orbits. An interesting paper by MR. CURLE dealt with the development of the hut circle in Scotland, beginning with Neolithic—for which, however, there was little evidence—and ending with Christian times. DR. JAMES RITCHIE brought forward evidence for the existence of Palaeolithic man in Scotland from recent excavations in limestone caves at Inch-nadamph in Sutherlandshire, in which the associated evidence—geological, palaeontological and anthropological—determined the date of the various layers and pointed to the presence of man of Upper Palaeolithic Age. MR. GRAHAM CALLENDER adduced proofs of two distinct land movements in Scotland since Azilio-Tardenoisian times. The Azilio-Tardenoisian was contemporary with the 25–30 ft. raised beach; subsequently a rise took place, followed by a sinking which was still going on. The argument was supported by reference to monuments from Neolithic to Romano-British times. CANON MACCULLOCH dealt with the Picts, actual and traditional, and endeavoured to trace the folk use of the term to its origin. The REV. J. M. MCPHERSON produced a remarkable record of primitive beliefs in the North-East of Scotland, and the REV. A. C. MCLEAN showed how a folk-tale of Callernish, known to go back to 1770, when it was already old, corresponded exactly with the results of archaeological research.

The archaeology of various periods in the rest of Britain and other parts of the world was well represented. MR. LESLIE ARMSTRONG'S report on his further excavations in the Derbyshire caves included a description of the "engraving of a masked human" possibly of reindeer, resembling the art of Altamira. An important and in some aspects illuminating communication by DR. ARTHUR RAISTRICK and MISS S. E. CHAPMAN described the Celtic lynchet systems of Upper Wharfedale, Yorkshire, an area in which the strip lynchet fields are almost perfectly complete. The parish of Kilnsey with Comstone afforded an example of an Anglian village organisation, which lasted through the Norman period down to the fifteenth century.

Roman Britain was represented by MR. S. N. MILLER'S account of the excavations of Roman York in the current year, during which a number of novel features have been brought to light. DR. R. E. MORTIMER WHEELER was, unfortunately, prevented from attending to describe this season's work at Caerleon.

Turning to archaeology in other fields, mention must be made, in the first place, of MISS GARROD'S excavation of the cave of Shurbah in Western Judæa, which have brought to light, in a lower level of Mousterian

age, several fragments of Neanderthal man, while an upper level contained a microlithic industry of a type so far unknown in Palestine, with a number of human skeletons. MR. H. FIELD also dealt with the Palaeolithic period in his account of expeditions in the Syrian desert in 1927–8, in which discoveries of Palaeolithic implements pointed to the desert being a highway rather than a barrier for prehistoric peoples. MR. MILES C. BURKITT'S survey of prehistory in South Africa was followed with close attention and interest in view of the forthcoming Meeting of the Association in South Africa.

A number of communications dealt with the archaeology of the Near and Middle East. SIR FLINDERS PETRIE gave a summary account of the work of the British School of Archaeology in Egypt since taking up work in Southern Palestine, a border area which the author considers is likely to throw much light on Egyptian history. The excavations in 1927–8 of the joint expedition of Oxford University and the Field Museum of Chicago at Kish were described by MR. H. FIELD, while MISS M. A. MURRAY discussed the "Fountain of Death," with numerous monuments. MR. W. HEURTLEY, himself unfortunately unable to attend, sent a communication which was presented by PROFESSOR V. GORDON CHILDE, in which he described his further excavations on behalf of the British School of Archaeology at Athens in Macedonia. During the present summer the village of Bouboustia in Western Macedonia, a small site of the Late Bronze and Early Iron Ages, was excavated, yielding pottery of a type similar to that found in a whole series of sites, of which certain characteristics may be regarded, wherever they are found, as proof of identification by Macedonian tribes.

CHILDE discussed the origin of certain Hallstadt types, ending on a note of interrogation whether it might not be legitimate to regard the diffusion of these types as due to movements of Illyrian tribes from west of the Middle Danube plain. MR. O. DAVIES dealt with the bearing of the recent discovery of tin mines near Delphi upon the question of the source of tin in pre-historic Greece.

A session was devoted to ethnographical papers. MR. MCILWRAITH'S account of secret societies of the North-West coast of America attracted a large audience; while MR. GRAHAM KERR'S description of the important collection of Japanese antiquities made by DR. GORDON MUNRO, and now in the Royal Scottish Museum, was the first account of the collection as a whole which has been given to the public. CAPT. G. E. H. WILSON'S paper had a twofold interest, in that it described an old agricultural system in Uhehe, Tanganyika, and attempted an identification of the coast line of Palaeolithic man in particular of Rhapta. MR. H. FIELD'S paper on the hunting tribes of Kenya, in the absence of the author, was taken as read.

In conclusion, it may be added that while the Sectional programme attracted large audiences, a heavy list of exceptional interest precluded anything like the adequate discussion which would have been welcome. E. N. F.





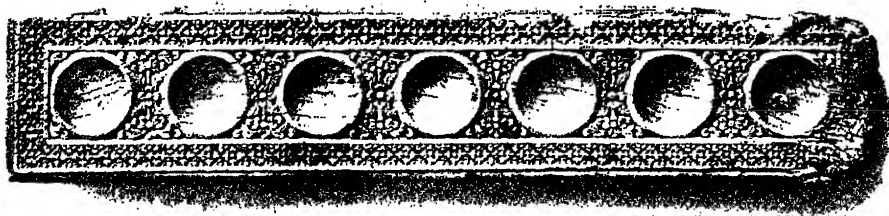
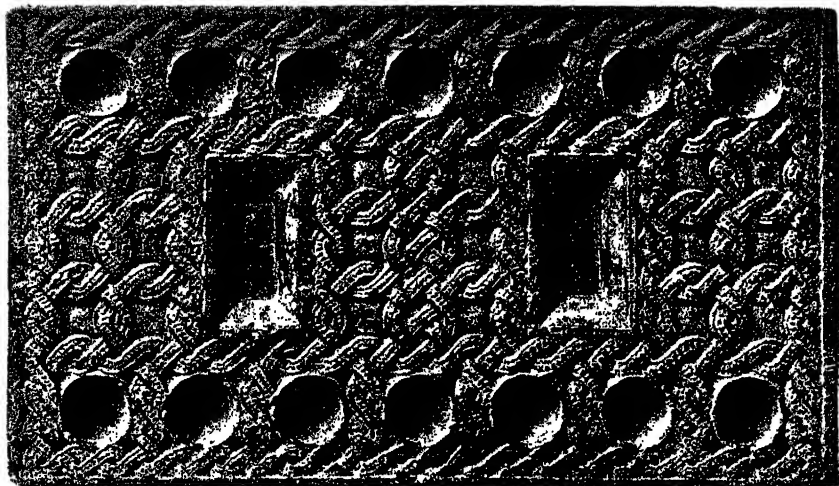
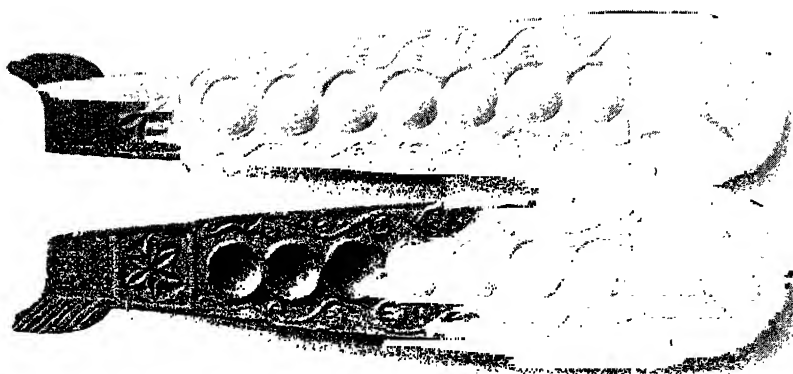


FIG. 1.



FIG. 2.



FIGS. 3 and 4.

PALLANGULI: A SOUTH INDIAN GAME.

## ORIGINAL ARTICLES.

With Plate L.

India : Games.

Durai.

**Pallanguli : A South Indian Game.** By Mrs. H. G. Durai. With Plate L. **135**

In South India there are several variations of a game known in Tamil as *pallanguli*, or "Manyholes." In view of the fact that it resembles—superficially, at any rate—the universal African game, and that several notes on the African varieties have recently been published, the following description may be of interest.

*Pallanguli* is played on a wooden board which may be quite plain or may be beautifully carved and ornamented (Plate L, Fig. 1). The board has two parallel rows of holes scooped out. The number of holes varies, but in the particular variation which I propose to describe there are seven holes each side. Sometimes at either end of the board is a large hole for holding the captured pieces. The stones of the tamarind, tiny pebbles, or small cowrie shells are used as pieces, the last-mentioned being preferred because they make an attractive sound as they fall into the holes in course of play.

The two players sit down with the board between them and begin by placing six pieces in each of the seven holes on their own side.

The first player picks up the pieces from any one hole on his own side, and, moving always in a counter-clockwise direction, places one piece in each of the holes as he goes, leaving, of course, the hole from which he picks up the pieces empty.

Having deposited the last piece in a hole containing others (either on his own or his opponent's side) he picks up all the pieces in the hole next the one he ended in and proceeds as before still in the counter-clockwise direction. The move ends when he puts his last piece into a hole next to an empty one. When he does this he captures all the pieces in the hole on the further side of the empty one in the clockwise direction. If this hole is also empty the move ends, but he captures nothing.

It is now his opponent's turn to play. He may start anywhere on his own side, and, moving in the counter-clockwise direction, he proceeds in the same way until his move ends in a capture or a blank. Each player plays thus in turn until the board is cleared. During the course of the game empty holes gradually become filled, and as soon as the number of pieces in a hole becomes "four" the players on whose side the "four" is removes them from the board and adds them to the pieces already captured. A "four" is called a *pasu*, which means a "cow," which is interesting as being a probable survival from days when, as in Africa, the game was a parody of cattle raiding. At the end of the first round, when the board is cleared, each player puts back his winnings, six to a hole, into as many holes on his own side as he can fill. The loser of the first round will not be able to fill all his seven holes, and into the empty holes he puts a bit of paper or leaf or other rubbish to show that it is not in use, and these holes are known as *peekkuli*, or dung holes. Any pieces left over less than six are retained. The winner of the last round fills each of his seven holes with six pieces and, of course, will have a balance of few or many pieces in proportion to the number of holes his opponent has been unable to fill.

The right to play first alternates round by round, and who ever did not begin last round starts off on his own side before, and the game proceeds exactly as in first round, except that the rubbish holes are left out.

The game goes on until eventually one or other of the players has not even enough pieces to fill one hole. During any round by skilful play it may be possible to capture sufficient pieces to enable the player to reopen one or more rubbish heaps,

and even, of course, turn the tables on the other, and, consequently, a great number of rounds may have to be played before the game is won outright.

*Pallanguli* is really a women's game in South India, and they play it when the morning's work is finished. Men do sometimes play it as a gambling game.

I believe there are penalties for cheating, but I am not certain, as I do not remember ever having incurred any.

No deliberate counting of pieces before a move is allowed, but observant and experienced players can tell at a glance what is the best move. H. G. DURAI.

#### EXPLANATION OF PLATE L.

Fig. 1. Half of an ivory *pallanguli* board. India.

Fig. 2. Wooden Board for *Olinda Poruwa* (= *pallanguli*) in the form of a fish. Ceylon. (Neville Collection, British Museum.)

Fig. 3. Wooden Boards for *Olinda Poruwa* in the form of a fish. Ceylon. (Neville Collection, British Museum.)

Fig. 4. Wooden Board for *Olinda Poruwa*. Ceylon. (Neville Collection, British Museum.)

### Ireland: Archæology.

Whelan.

#### The Implementiferous Raised Beach Gravels of South-East Antrim. By C. Blake Whelan.

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The contrast between the flint industries of the 25 ft. raised beach of north-east Ireland and those of the corresponding Scottish deposit has been often noticed. Yet it is not unreasonable to believe that, when we have excluded the more obviously derived material, some at least of the respective culture stages here represented may be contemporaneous, and for that reason the dissimilarity in technique and types is the more interesting. Both the Scottish and northern Irish beaches are in places based upon submerged peat, and there are grounds, geological and palæontological, for attributing the position of these beds to the general depression which in southern Scandinavia was responsible for the Littorina Sea. On this view the derived artifacts recovered from the lower gravels may be at least epipalæolithic, and may relate to the earlier period of the Ancylus lake and submerged forests, during which the transitional industries set in and the remnants of the hunting races scattered to the north. Unrolled or less travelled flints, deposited while in use or manufacture as the depression continued, might thus be taken as more strictly representative of the neolithic dawn. In addition to the Scottish Azilian types, we have the authority of l'Abbé Breuil for a La Madeleine date in the case of certain flints from the Campbeltown raised beach. Correlations of the early neolithic industries are notoriously dangerous, and, no less on that account, it is desirable that every gleam of light obtainable from the evidence should be focussed on this obscure subject.

In an able paper published after his premature death,\* Dr. Walter Bremer suggested that the rough Islandmagee (S.E. Antrim) flints collected by the late Mr. W. J. Knowles owed their characteristic forms to the Asturian culture contemporaneous in Northern Spain with the Campignian. These Dr. Bremer took to represent the earliest Irish artifacts—Sligo had not then loomed—and he dismissed incidentally Knowles's case for the crudely flaked pebbles from the eroded *débris* of the raised beach. This latter group is believed by others to represent a primitive type of batiform palæolith, and I hope to take an early opportunity of publishing additional examples. Certain negative arguments were used by Bremer with much force in the development of his thesis. It cannot be too strongly emphasised, however, that his contention rested on typology and that the material from which his illustrations were drawn was not obtained *in situ*. The *pics* and

\* Proceedings of the Royal Irish Academy, Vol. XXXVIII, Section C, No. 2.

*coups de poing* figured in the first instance by Knowles\* were recovered by him in the gravels scattered by recent marine encroachment along the shores of south-east Antrim, and it is very evident that the history of the transported specimens secured from time to time from the surviving sections began long before their arrival in the deposits where they have been found. Bremer found an analogy between certain of the Islandmagee flints and the Nöstvet axe, but this is not a leading type of the Asturian. He discounted this *prima facie* indication of a Norwegian derivation, however, by pointing to the absence of the type in Scotland. He also laid some emphasis on the "extreme rarity" of the *tranchet* in the Islandmagee area, although in Knowles's last paper eight excellent specimens are figured, of which no less than five are actually from the narrow confines of Islandmagee itself, and the others from the raised beach areas elsewhere. Moving inland to the famous cultures of the river Bann, Bremer was impressed by the recurrence of characteristic hand-picks identical in point of technique with those from the coast, but he is surprisingly silent as to the 82 *tranchets* collected by Knowles from the Bann neighbourhood. Pressing home his denial of a Campignian origin, Bremer alludes to the total absence of the *coup de poing* in Campignian deposits and turns convincingly to the Asturian hand-pick, a long point worked from a rolled pebble



FIG. I.

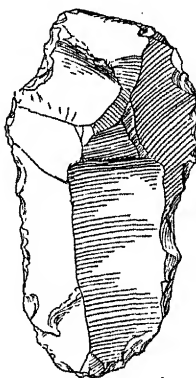


FIG. II.

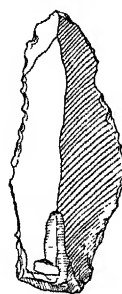


FIG. III.



FIG. IV.

by rough retouches, as the analogue of the northern Ireland implement. The theory, characteristic of Bremer's brilliance as an archæologist, is certainly attractive, but unhesitating acceptance on the sole ground of an alleged technical affinity seems undesirable. The pick is not the best criterion of neolithic origins: it was too persistent a form. Such

implements were made in the Rathlin diorite factories and I have myself found examples in that island closely comparable with the early neolithic types on which Bremer sought to found his contention. While it might be suggested that these in turn derived from Islandmagee, nothing could be more dissimilar than the flint industry with which I have found them associated, *in situ*, between the surface deposits and the boulder clay. Indeed, this older Rathlin Island industry seems to have much in common with that of Aarhus, Jutland.

Where so much is geologically obscure, we can advance only by the sense of type gained in unremitting field-work, and the neolithic implements of northern Ireland are not so atypic as some have imagined. This fact was understood by none so well as Knowles, and we have hitherto been the poorer for our failure to appreciate him as a litho-classiologist. While maintaining his belief in an Irish

\* Proceedings of the Royal Irish Academy, Vol. II, 2nd Ser., No. 5.

† Proceedings of the Royal Irish Academy, Vol. XXX, Section C, No. 1.

Palæolithic, Knowles confined himself in his more mature studies to the indications of earlier and later neolithic occupations of the country, thus distinguishing between the late culture of the sandhills and the primitive industry which at one stage included the *coup de poing*. The best example of this hiatus is available at Whitepark Bay, County Antrim, where I have recently added to his series of ochreous implements which had been found and rechipped by the later race. The first-comers of this latter people made their arrowheads with a straight or slightly incurved base and these I have recovered in the oldest black layer, beneath 20 ft. of blown sand, associated with roughly baked pottery exhibiting the crudest geometric decoration. While adopting this essentially cautious basis, Knowles showed himself alive to the derived character of the *coups de poing* from the marine gravels. Nothing better illustrates the chaotic thought of local antiquarians than their obstinate insistence that these implements had been recovered from the surface and had not been found within the body of the gravels. This misconception is not yet fully dispelled, and it may therefore be opportune to give one or two instances of implements which I have recently found in the best surviving sections of the Larne raised beach. These remaining deposits are in places 14 ft. in depth. The specimen sketched in Fig. 1 was obtained at 12 ft, and is an excellent example of the thick flake implement made on the Levallois principle. The upper surface illustrated is composed of several long truncated flake scars, while the under surface exhibits a plain bulbar face. The original flake was reduced by rough and heavy resolved flaking to a blunt point at the bulb. The opposite end has been slightly faceted and left broad and heavy for gripping in the hand. The ridges and outstanding parts are not only much rounded by rolling, but these prominences and portions of the flaked surfaces have been attacked by the weathered incrustation characteristic of the transported material in the gravels. The patination is a deep cream and porcellanous, no cortex being left. There can be no doubt that the implement is one intentionally dressed as a hand weapon. In a district where flint manufacture must have proceeded from the earliest human occupation, the provenance and characters cannot be regarded as confining the period to that of a Neolithic culture proper, however remote.

The remaining sketches are of small flakes from the 9 ft. level. These are found also at lesser depths throughout the gravels and they present certain features worthy of special comment. The patination is pure white and the ridges and surfaces, though more or less rolled, are quite free of weathered crust. Fig. II shows a blade with transverse flake scar and notching towards the end opposite the bulb. This specimen is a well-made spokeshave and compares closely with several in my collection from the older Rathlin Island industry. Figs. III and IV. are of narrowly trimmed flakes, the former showing "pinching" at the bulb end and a prepared point at the other. Of this form I have a Larne series which offers comparison with the oldest river Bann material. It has been said that the Bann industries present Campignian features, and for the most part the river implements are ochreous and slightly rolled, while they also occur in surface deposits for some distance through the valley. They include many broad-winged flakes and more slender fish spear points of careful symmetrical outline. There is, however, what appears to be an older group of which representatives are occasionally brought to light in dredging. These are of a lustrous dark brown or black patination and have been much rolled in the stream bed. A leading type is the narrowly trimmed pointed flake with pinched stem, and I have obtained a series of such artifacts which can be matched, specimen for specimen, with those from Larne. Such identity of workmanship can hardly be fortuitous, and I believe that it supplies the answer to Knowles's question regarding the source from which the older inhabitants of the Bann valley drew their flint supplies. In the technique of the edge-trimmed flake

we may possess a hint of the neolithic sequence which has in the past been overlooked. Irish surface implements of this sort abound in numerous haphazard collections, but to an experienced field worker they bear distinctive features by which they can be readily grouped in point of provenance, outline and patina. When we have gained at least an inkling of the Irish neolithic succession, speculation upon the more remote origins may prove of greater profit.

C. BLAKE WHELAN.

### Africa, East: Religion.

Beresford-Stooke.

#### An Akamba Fortune Telling-Ceremony. By G. Beresford-Stooke.

137

In former days, when a raid was proposed on the neighbouring Masai or Akikuyu, it was usual to consult a witch doctor as to the prospects. He would also be consulted in cases of sickness or on undertaking a journey of importance.

The medicine man brings with him a gourd or calabash containing a large number of small seeds (Kikamba, *mbuu*; Kiswahili, *m'baruga*). Sitting down, he spreads in front of him a leopard's skin. He then takes his musical bow (described by Lindblom in "The Akamba," cap. XXII, section 1), and putting his lips to the calabash, sings for a few moments, invoking the aid of the spirits. Then, shaking the calabash, he suddenly jerks some seeds out of the narrow mouth on to the leopard's skin. These are counted out into tens. If the result is a multiple of ten, the seeds must be thrown again. If the number left over is less than ten, the fortune is as follows:—

- |       |   |   |  |
|-------|---|---|--|
| One   | - | - | Bad.   |
| Two   | - | - | Doubtful. In the case of a woman about to start on a journey, if she walks with her left foot, and hurts that foot against a stone, she must turn back. If she hurts her right foot, she must hurry on quickly. If she walks with her right foot, the reverse applies. A woman is said to walk with her left foot if she has borne a daughter, and with her right foot if she has borne a son. |
| Three | - | - | Good for everything, as one puts three stones under a cooking pot.   |
| Four  | - | - | Bad. A sick man will die. Good for a woman.  |
| Five  | - | - | Good. A sick person will recover.  |
| Six   | - | - | Good for a woman. Bad for a man.   |
| Seven | - | - | Good for a man. Bad for a woman.   |
| Eight | - | - | Good for a woman. Good or bad for a man, according to circumstances.   |
| Nine  | - | - | Bad. If a woman is pregnant, she will bear a girl, not a boy.  |

Fortune-telling methods are described extensively by Lindblom in "The Akamba," cap. XIV, section 2, but the significance of the numbers is not recorded. In cap. XIV, section 13, Lindblom refers to the odd numbers as being a good omen at a divination. I am told that the best number is three and the next best five. The two worst numbers are one and nine. The odd numbers, therefore, contain both the best and the worst.

At the same time, a clever medicine man can, if the inducement be sufficient, find an excuse for turning an unlucky number into a lucky number. It should be remembered, too, that no two medicine men will follow exactly the same procedure.

G. BERESFORD-STOOKE.

### Africa, East: Religion.

Huntingford.

#### Note on Some Names for God. By G. W. B. Huntingford.

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In addition to the official names for God, *Asis* and *Cheptalil*, the Nandi have a sort of private name, which is *Chep-op-koi-yo*. In this word, *chep-* is the common formative prefix; *-op-* a variant of *ap*, "of"; and *-yo* a suffix. The

word is built up on *-koi*, which I believe to be the same as the Masae (*eng-*)*aĩ*, "god." I would refer both the Nandi *koi* and the Masae *aĩ* to the Bari *ga*, "guard" (*see* Mitterutzner, s.v. *Ga.*). The meaning of both would therefore be "guardian." For the insertion of *k* (*g*) in Nandi and Bari, *cf.* Nandi *koĩg*, eye; Bari, *konge*; Masae, *oĩgu*; Laĩgo and Shilluk, *waĩg*.

G. W. B. HUNTINGFORD.

### Africa, East: Linguistics.

Huntingford.

**Note on the "Taturu" Language.** *By G. W. B. Huntingford.*

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In Last's "Polyglotta Africana Orientalis," p. 188, is given a vocabulary of a language described as "Taturu," spoken a little north of Rangi, in Tanganyika Territory (lat. 5° S.; long. 35° E. approx.) Though the words are very inaccurately taken down, it is evident that the language is a dialect of Okiek (Dorōbo), as the following table shows:—

Taturu.	Tindiret.	Dorōbo of		Nandi.	Kony.
		Ravine.	Digiri.		
ailut: river.	ilosto				
badaiit: back.				patai	
beek: water.			pek	pek	pêko
budiega: hair.			putek		putek
it: ear.	iit			iit	iit
kilukt: bull.				kiruk (kirkít)	kiruk
kut: mouth.	kut			kut	kut
laganet: stones.				legemet (hill)	lagemet
nduk: cow.	tuka (pl.)			tuka (pl.)	tuka (pl.)
nyenyit: down.	iĩgunyinyek	ngwon		iĩguny	iĩgwen
sereta: nose.	serut		serut	serut	serut
tarietend: bird.	tarityet			tarityet	taritit
uwenda: bow.	kuyaĩgda	kwianda	kwianda	kwang	kuyaĩgda

The Taturu words are here given as Last spelled them; the words with which they are compared will give some idea of their proper form.

G. W. B. HUNTINGFORD.

## Archæology.

Childe.

**The Origin of Some Hallstatt Types.** *By Professor V. Gordon Childe.*

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The "Hallstatt" culture, as generally understood, results from the diffusion of certain series of types. In many cases it was mixed with or adopted by pre-existing local groups such as the urnfield culture of East Central Europe (Platenice), or of the North Alpine slopes (Gündlingen), or the tumulus builders of Western Bohemia and South-West Germany (Salem-Alb). P. Reinecke indeed extends the term Hallstatt to embrace many of the earlier local cultures, but it is better to limit its content to those which have absorbed certain specific types. Of these, the "Hallstatt sword" and certain fibulæ are the most important, and here we shall take the safety-pins as our principal guide.

The most distinctive is the spectacle brooch. A study of its distribution alone would suggest that the original focus lay just east of the mountain chains that run parallel to the Adriatic from that Austrian Alps southward to the Peloponnese. Hence it spreads (a) northward\* into Switzerland, Bavaria and Bohemia and down the Rhine to the Main; (b) north-eastward\* across Lower Austria and Moravia to the Oder Valley, and (c) eastward along two routes—(c<sub>1</sub>) across Pannonia to the Middle Danube and—very sporadically—up the Maros†, and (c<sub>2</sub>) to Macedonia‡ and Bulgaria.§ In groups (a) to (c), apart from

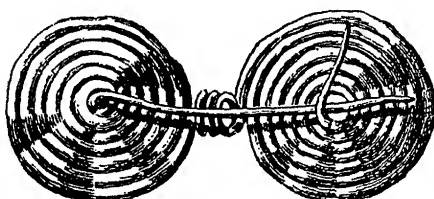


FIG. 1.—PRIMITIVE SPECTACLE BROOCH,  
MARIA RAST.  $\frac{1}{2}$

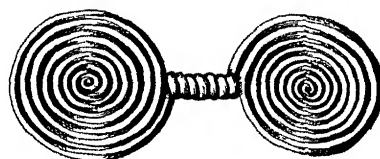


FIG. 2.—DOUBLE SPIRAL ORNAMENT,  
PROTSCH, SILESIA.  $\frac{1}{2}$

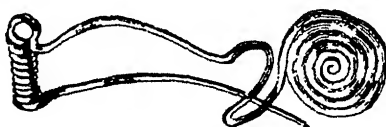


FIG. 3.—PRIMITIVE HARP FIBULA,  
MARIA RAST.  $\frac{1}{2}$

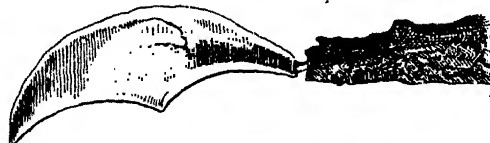


FIG. 4.—KNIFE, MARIA RAST.

one or two curious variants (such as specimens with a separate pin in "Nordic" style||), we have only the standard form with a little "figure 8" twist between the two spiral discs. In Macedonia a simpler form without the "figure 8" is also represented; this type is characteristic of the great cemeteries at Glasinac¶ in South Bosnia, and gives us a clue as to the immediate starting-point of the Macedonian-Bulgarian series. It is clear that the two varieties were differentiated very early, since the normal form never took root among the communities living round Glasinac.

The prototype of both varieties is revealed by a grave in the urnfield of Maria Rast in Styria. It contained a spectacle-brooch\*\* in which the two spiral discs

\* *Z. f. E.*, 1913, pp. 785-9.

† *Arch. Ert.*, xxxi (1911), pp. 348f.

‡ Casson. "Macedonia," pp. 141ff; *P.Z.*, ix, p. 66.

§ *Izv. Bulgar. Arch. Druzh.*, iii, pp. 291f.; *Izv. Bulgar. Arch. Inst.*, 1921-2, pp. 31ff., 76ff.; 1924, pp. 105ff.

¶ In Plzen Museum from a tumulus in the vicinity.

¶¶ *W.M.B.H.*, xi, p. 97.

\*\* Much, *Kunsthistorisches Atlas*, pl. XL, 5.



are united by a short spiral coil (Fig. 1). Objects of exactly the same form, but without the pin, have been found in hoards or graves belonging to the Lausitz culture (probably its second phase) in Silesia\* and Moravia,† and again in Hungary and Bosnia‡ (Fig. 2). In all cases the context of the objects seems to be Late Bronze Age (Reinecke's phase D, or at latest his Hallstatt A). My suggestion is that the spectacle brooch was created by the attachment to these objects of a pin under the inspiration of older fibulæ, the Maria Rast specimen representing a transitional form. Its creation must have taken place somewhere between the northern limit of pure Bronze Age one-member fibulæ—a line from Munich to Vienna—and the southern limit of the spectacle coils, which lies perhaps on the Narenta.

(2) A fibula very common in oldest genuine Hallstatt cemeteries of Lower Austria, and sometimes found in a similar context in Moravia, Eastern Bohemia and Silesia, is the "harp fibula."§ It is obviously derived|| from the simple violin-bow fibula with spiral catch-plate through an enlargement of the catch-plate represented in a couple of fibulæ from North Hungary¶ and a subsequent extension of the spring. Maria Rast again offers an intermediate form in which the spring, although elongated, has not attained the length of the standard Hallstatt form (Fig. 3). The focus accordingly lies just south of the Austrian Alps.

(3) The eastern spread of Hallstatt forms to Transylvania and still more to Macedonia and Bulgaria,\*\* brought thither variants of the simple bow fibula with a loop just above the catch plate (*zweischleifige Bogenfibel*). The same type occurs at Hallstatt, already in cremation graves, but not further north. There are early specimens in Bosnia and Greece,†† and another equally early example from Maria Rast in Styria. Its focus must, therefore, lie south of the Noric Alps.

Surveying the distribution of these three types, which must be early, both relatively within the "Hallstatt" period, and, on the Macedonian evidence, also absolutely, we get the impression of an early proto-Hallstatt culture at home in the triangle between the Noric and Julian Alps, and near its western edge, where influence from the Adriatic was strong.

A consideration of the swords confirms this conclusion. A type distinctive of the last phase of the Bronze Age was the antennæ sword whose home lay probably in Switzerland, but in any case north of the Noric Alps. It occurs rarely in Italy, and again in an early barrow at Podsemel in Carniola.‡‡ Yet this definitely north-western type was brought to Macedonia§§ in company with the western fibulæ described above. The typical Hallstatt sword and its close kin, the Glasinac series, must have developed a little later, but again between Glasinac and Hallstatt.

Iron seems to have reached this area from the Adriatic, since perhaps the earliest iron object in Central Europe is the handle of a bronze knife of Benacci I form from Maria Rast (Fig. 4). But the new metal was in use in Kizwadana, and even Greece, long before Hallstatt times—even the iron swords from Halos are pure Bronze Age forms. Hence Hallstatt is better defined by types such as have been enumerated above.

\* S.V., iv, p. 33 (Protsch).

† Gottwald. *Pravěka Sídliště a Pohřebiště na Prostějovsku*, pl. ix, 3 (Hrubčic) *Lf. Pravěk*, 1908, p. 100, Fig. 17, i; *Schráníl, Vorgeschichte Böhmens und Mährens*, pl. xxviii, 23.

‡ W.M.B.H., i, p. 150; vi, p. 541.

§ Z.f.E., 1913, pp. 782-4. Note especially the inhumations at Adamowitz and Bylan.

¶ Beltz, *ibid.*, p. 681.

¶ Mártón, *Arch. Ert.*, xxxi, p. 336, Fig. 3.

\*\* See Notes †, ‡ and § on preceding page.

†† Blinkenberg, "Fibules grecques et orientales." Figs. 69-70.

‡‡ M.A.G.W., xxx (1900), p. 48, Fig. 1.

§§ Casson. "Macedonia," Fig. 50.

The earliest traces of this culture there are found in urnfields like Maria Rast, Hallstatt and Stillfried on the March, which are in the long run generically connected. There are, however, indications that the diffusion of these types was not due to the "urnfield folk." The earliest Iron Age graves of Bosnia, Macedonia and Bulgaria all contain inhumed bodies. That is noticeable also in the first graves with Western types along the Marós.\* In the Bylan group† of Central Bohemia, too, and even in the urn-fields of Moravia and Silesia,‡ the appearance of distinct Hallstatt forms is associated with occasional inhumations. In South-West Germany also, cremation is no longer the exclusive rite in early Hallstatt times. In Transylvania the graves quoted are furnished with horse-gear; and in Bohemia and Silesia, too, the early horseman's burials were by inhumation.

Both in Macedonia§ and in Transylvania|| the Early Iron Age pottery includes vessels with thumb-grip handles—a type that is notoriously at home in Illyria. But it is also found in early Hallstatt barrows in Pannonia and Styria,¶ and—perhaps rather later—in Bohemia.\*\*

Finally, Schliz†† was led by an examination of the skulls from Hallstatt barrows in South-West Germany and Bohemia to postulate an immigration from the south-east—from Illyria.

In the light of all this, we might infer that the diffusion of the classical Hallstatt culture was largely due to infiltrations of equestrian adventurers from Pannonia, who made raids in various directions and sometimes settled down as petty barons among alien people.

V. GORDON CHILDE.

## REVIEWS.

Italy: Archæology.

Sundwall.

*Villanova-studien.* By Dr. Johannes Sundwall, in "Acta Academiæ Aboensis." Abo, 1928. 141

In the unpretentious form of a paper contributed to the publication of his faculty in the University of Abo, Professor Sundwall presents us with an important study on the Early Iron Age. To the preparation involved in his earlier essay on Hut-urns he has now added the results of much patient labour in the museums of Bologna and Este, and he has studied with very unusual care and minuteness all that has been written by other authors on his subject.

Whether the Villanovan culture originated in the north and spread southwards or whether its course was in the opposite direction is, as this author says, a question long and hotly debated. Grenier, von Duhn, Ducati and various others have expressed divergent opinions, all grounded upon different interpretations of the same evidence. My own treatment of the problem is to be found in my "Villanovans and Early Etruscans." Professor Sundwall now brings a fresh contribution to the debate, based principally upon his conception of the evolutionary

development of forms and patterns. As his memoir is not illustrated, except by a few rough pencil sketches, he relies for his pictorial material upon references to Grenier's "Bologne villanovienne et étrusque," Ducati's catalogue of the Museo Civico at Bologna and my own volume already mentioned. The reader who wishes to follow the closely-knit arguments on Bologna and Etruria with proper attention will need to have these three books in front of him all the while. The sections dealing with Este, on the other hand, are rather inadequately served by references to several volumes of the "Monumenti Antichi" and the "Notizie degli Scavi."

The treatment of Bologna opens with a discussion of the San Vitale and Savena cemeteries and their relationship to the Terramare. Sundwall maintains that no part of these cemeteries, which are undoubtedly the most ancient found at Bologna, need come down beyond the close of the First Benacci period. This agrees with my own observations, though Ducati, who still holds Cæsar's Will in the form of the unpublished material, has always maintained, without producing his evidence, that some of the graves are full Second Benacci. So much of the argument in this memoir is

\* E.g., Maros Csapá, *Arch. Ert.*, xxix (1909), p. 166.

† Piž, *Česke před.*, i, pts. XXV and XXXIV.

‡ E.g. *S.V.*, 1892, pp. 206ff.; *Reallexikon*, ii, p. 96.

§ Casson, *l.c.*

|| *Real.*, viii, p.

¶ *M.A.G.W.*, xv (1885), pl. VIII (Wies in Sulmtal).

\*\* Piž, ii, pl. XXV, 8; *M.A.G.W.*, xxxv, pl. II.

†† *A.f.A.*, ix (1910), p. 245.

based upon the high antiquity of these two cemeteries that it is most desirable Ducati should lose no time in publishing, as he has promised, the full and complete record of the excavations. Sundwall's conclusions as to the derivation of the Villanovan ossuary are very sound, if not wholly new. He traces it to originals manufactured in Central Europe, but does not directly connect it with the Terramare. Rather, as he says, there were original elements, such as the *Schnurkeramik*, common to each of the two cultures. The decorative *répertoire* of the earliest Villanovans he finds to be almost wholly distinct; and with laudable independence he refuses to follow Böhlau in tracing it all to an ancient Greek source. He considers that it must have originated near the Danube, which was the common home of all the geometrical school. I am so fully in agreement with all this that I will not yet disturb our harmony by interjecting an objection to the date proposed for the migration. This, together with some other points of absolute chronology, shall be treated in a separate article in the next number of MAN.

The next section, on "die ältesten Villanovans und ihre Kreise," is followed by three others (pp. 18-56) which contain a discussion of the typological development of pottery and bronze vessels together with their ornamentation during the three periods, Benacci I, Benacci II, and Arnoaldi. Absolute chronology is discussed on pp. 57-62, while a valuable section, pp. 62-76, treats of the relations between Bologna and Etruria. Finally, there is a long discussion on Este, to which I must devote particular attention.

Sundwall's principal guiding-line through the mazes of Villanovan development is the evolution of form in the pottery and bronze vessels, particularly the ossuary, which, as he correctly remarks, I omitted to emphasise in my "Villanovans and Early Etruscans." It will better explain a certain reserve which I cannot help maintaining towards some of his deductions if I say that this omission on my part was deliberate. For in spite of the large number of tombs at Bologna the variations of form are very few, at least in the First and Second Benacci periods, and do not lend themselves at all naturally to seriation. Where there are so many gaps in the logical sequence, and so many loose ends in the tangled skein, it is difficult to be quite convinced that even the most ingenious of weavers has joined the threads according to the original scheme. Let me at once add, in mere justice, that Sundwall has made a remarkably effective use of his method, and has brought forward many new and very striking points. Especially in regard to the relative precedence of Bologna and Etruria his reasoning is extremely telling, and he succeeds in producing a very consistent picture.

We may now consider very briefly the principal contents of his sections on the Bolognese periods. The bronze ossuary found occasionally in the Second Benacci tombs at Bologna he shows to be the prototype of a whole series of pottery ossuaries. He maintains that this bronze ossuary is an importation

from Este. The suggestion is attractive, but not quite convincing, as the actual form does not occur at Este.

More logical is his derivation of bronze-studded pottery from the well-known and abundant bronze-studded ware of Este; though the possibility of connection with the Faliscans must be remembered. That the situla-form evolves independently both at Bologna and Este and is not in either case borrowed from Etruria, I entirely agree. In regard to decoration, he connects some of the ornament on bronze girdles with the Hallstatt tradition, but points out that it is also affected by the patterns of textiles. On chronological and other grounds he refuses to accept my suggestion that the occasional painted ware was derived from the Faliscans. All these arguments deserve close study, for the exact nature of the interrelation between Bologna and Este is exceedingly difficult to determine. Each place displays a powerful originality, and each influenced the other. We may come nearer to defining their relationship by precisely this kind of close analysis.

Space forbids me to discuss Sundwall's careful treatment of the Arnoaldi period, which adds many details to my own short characterisation, but changes no essentials. The remarks on the bronze ossuaries of this stage are important, and so is the demonstration that much of the pottery ornament is derived from bronze work. The motives on the pottery are studied in very full detail, and the influence of Greek geometrical ware, obtained through Etruria, is duly emphasised. Reserving for future treatment all discussion of the section on chronology, we may pass on to pp. 62-76, which deal with the cultural relations between Bologna and Etruria.

In his treatment of Volterra, Vetralla, Marsiliana and Populonia there is little new or remarkable, and nothing to which exception can be taken. For Vetulonia Sundwall handicaps himself by making the foreign imports necessarily dependent on transmission through Greek trade, and therefore subsequent to Greek colonisation, an unnecessary limitation. His most interesting experiment in this section is a typological grading of bronze vessels. But whether this grading has any chronological validity may well be doubted, when we see that Sundwall has to admit that the very latest typological development, a bronze vessel from Val di Campo, actually occurs in the Benacci-Caprara stage at Bologna. There is certainly no reason for accepting the axiom that "Solehe Kriterien sind natürlich bindender als Schmucksachen die sich durch längere Zeit vererben können."

For the Corneto cemeteries he relies too strongly on his peculiar view of the absolute chronology of Second Benacci; if this is not accepted, much of his reasoning will collapse. But his relative grading is quite acceptable, and the emphasis on the connection between the Warrior's Grave and Bologna is well judged.

The pages (76-112) devoted to Este were written before Sundwall could see my handling of this subject in "The Iron Age in Italy." There are some advantages in having two entirely independent treatments of the same

theme, and in many places they supplement, without conflicting, with each other. Faithful to the same system which he has pursued throughout, Sundwall bases his classification and his argument almost entirely upon the development of the pottery and the patterns. So far as mere classification goes, this is quite unobjectionable, and there is much to be learned from his analysis and patient comparison of examples. But when he attempts to found a chronology upon this partial and incomplete basis he comes frequently into collision with results derived from a more catholic survey of the entire material. It would not be difficult to cite cases in which the typological evolution of the ossuaries and their patterns at Este does not march at all *pari passu* with the development of the fibulae, yet where the evidence of the fibulae is corroborated by all the rest of the tomb furniture.

The most important conflict of this kind occurs at the lower end of the scale where Sundwall is attempting to fix the limit in absolute time at the end of the Second Atestine period, or, in the terminology which he prefers, for the end of the "Impasto period." It is the moment when the banded red and black ware appears for the first time. To solve this question he most unfortunately invokes a fantastic theory of the derivation of the banded ware, and makes it the pivot of his chronological system. The theory is quite unnecessary, and the deductions from it are impossible. Briefly stated, Sundwall's contention is that the banded black and red ware of Este can only have been produced by a deliberate attempt to imitate the alternate light and dark bands of proto-Corinthian. I venture to think that such an idea could not naturally suggest itself to any unprejudiced observer who had never breathed the atmosphere of Böhlau and Johansen. This is not a matter of expert knowledge; it can be judged by any person with a clear eye, who has a reasonably wide experience of pottery, ancient and modern, and is not obsessed by preconceptions. Let any such reader look at the pottery in Plate 5 of my "Iron Age in Italy." The examples referred to are ornamented with horizontal bands of alternate black and red, separated by ridges of moulded clay. Does it seem, in the first place, a natural, or in the second place, a necessary, inference that these could never have been produced except as an imitation of proto-Corinthian? If the reader agrees with Sundwall, I must then go on to point out that all the historical evidence is dead against him. Sundwall candidly admits that no proto-Corinthian wares (or manufactures of any kind, he might have added) have ever been found in Venetia. I do not hesitate to go further and say that all the archaeological evidence goes to show that trade between Greece and Venetia did not begin before the fifth century; while there is no trace of indirect importation *via* Etruria or Apulia of either proto-Corinthian or Corinthian products. Yet on the sole authority of this fanciful theory of origin he puts back the beginning of the Third Atestine period to 650 B.C.; at least a hundred years too early!

In fixing the beginning of the third period at 500 B.C., I have been a little more rigid than Ghirardini, though my chronology is virtually interchangeable with his. Ghirardini placed the date at 450 B.C. and allowed an interval of uncertain duration for "transition" between the Second and Third periods. I have fused the "transitional" with the Third, allowing 50 years for the difference, which may be, perhaps, a rather scant allowance, but seems reasonable.

The real cornerstone for the dating of the Third Period is the tomb containing the Benvenuti situla. This bronze vessel is, as Ducati has conclusively shown, decorated with motives taken directly from the Certosa situla of Bologna. Now, a grave in the Certosa cemetery cannot *ex hypothesi* be earlier than the Etruscan colonisation of Bologna, which hardly antedates 500 B.C. There are, indeed, strong reasons for thinking that this particular Certosan grave is not earlier than 480 B.C. But I will not labour this point, duly stated by Ducati in his memoir on the situla, and only mention it to show that I am not overemphasising the case against Sundwall when I put the Benvenuti situla to a year not earlier than 500 B.C.

The next point to observe is that the tomb containing the Benvenuti situla is one of the very earliest in the Third Period. This is unquestioned; for it is contemporary with tombs positively classed by Ghirardini and Prosdocomi as "transitional" between the Second and Third Periods. Therefore the Third Period must begin at 500 B.C. The only good countering argument on the other side is that the practice of stamping patterns on the pottery, doubtless borrowed from Bologna, of the Arnoaldi period, occurs for the first time at Este in the Third Period. This is quite soundly reasoned, and I would further add that the fibulae from the tombs just preceding the Benvenuti are of late Arnoaldi pattern. I considered both these points, though not emphasising them in my text, when fixing the end of the Second Atestine period at 500 B.C., which is almost the last year of the Arnoaldi at Bologna. But my conclusion, which I still hold to be fully justified, was that the evidence of the Benvenuti tomb is overwhelming, and that it can be reconciled with the occurrence of Arnoaldi fibulae and stamped ware by supposing that these only appear at Este in the very last phase of the Arnoaldi. So that, whereas a slight extension of the Third Atestine period, to carry it back a generation or so behind 500 B.C., might be reasonable, it is quite impossible to carry it back to 650 B.C., because that is very near the actual beginning of the Arnoaldi.

Over the middle part of the Second Atestine period there is little inconsistency between Sundwall's treatment and my own. He adds many interesting suggestions which are very welcome. But we come again into conflict, though less seriously, at the upper limit of Prosdocomi's (and my own) Second Period, when Sundwall's typological sequences lead him to compare a certain number of the earliest tombs of Este to the First instead of to the Second Benacci of Bologna. Here, though I dissent, the disagreement is not much

more than a formality, because I fully admit that Este is rich in tombs of the very earliest Second Benacci, which would be sufficient for all the principal purposes of his argument. Nevertheless the point is worth making because I have insisted in my book that the First Atestine is at present an almost unreal abstraction. In my view, indeed, the First Atestine can hardly contain half a dozen tombs, and only one of these has any objects of interest. I devoted much attention at Este to studying this particular point, and came to the firm conclusion that it was impossible, on the analogy of everything known at Bologna, to transfer any of Prosdocomi's Second Period tombs to the First. The earliest, or one of the very earliest, Second Period tombs is No. 236, illustrated in Plate 2 of my "Iron Age in Italy," on which the student can form his own opinion. It distinctly belongs to the first stage of the Second Benacci (Benacci-Caprara) rather than to any part of the First Benacci.

Sundwall is, of course, fully justified in claiming tomb No. 2 in the museum as First Benacci, though I prefer to call it pre-Benacci; and it is quite arguable that the Lozzo tomb also belongs to the same phase. These furnish him with the parallel that he wishes for the San Vitale and Savena phase at Bologna. Otherwise, as in my own treatment, the First Atestine period virtually disappears, leaving a gap of several centuries between the close of the Bronze Age and the beginning of the Second Atestine.

I may conclude with one of the best examples of Sundwall's method, an induction which is genuinely valuable. In the San Vitale cemetery of Bologna is a specimen of a bronze-studded ossuary, unique and therefore not of Bolognese invention. It may certainly best be explained as an imitation of the technique so common at Este. Therefore, assuming always that San Vitale is never as late as Second Benacci, this technique must have existed at Este in the First Benacci period of Bologna. Hence we may infer that some day it will be found at Este in the First period, though as yet it is not known so early on that site.

There is, of course, no doubt that a First Atestine period actually existed, and that it was rich and important. The question as yet unsolved is whether it developed on Italian soil or in the original Danubian home. Nothing but future excavations can give the answer.

D. RANDALL-MACIVER.

Britain: Archæology.

Harrison.

*Harrison of Ightham.* By Sir Edward R. Harrison. Pp. xi + 395. 12 illustrations. Oxford University Press. Price 15s. net.

Sir Edward Harrison has given us, in "Harrison of Ightham," a history of the life of his father, Benjamin Harrison, and in so doing has placed many under a debt of gratitude to him. Benjamin Harrison was a very sturdy and well-known son of Kent. Not only did he live to a great age, but throughout the far-flung community

of prehistorians his name was a household word. For, though Harrison was an observer of Nature from many standpoints, yet his fame will rest upon his archæological researches. The neighbourhood of Ightham where he lived, like so many other parts of Southern England, is remarkably rich in the remains of prehistoric humanity, and Harrison possessed the genius for exploiting it to the full. When one considers the relatively small area over which he operated it may appear remarkable that the results he achieved are of such far-reaching significance. But, important discoveries are often made by those whose opportunities seem meagre, and the extent of their activities restricted. For all time it will be remembered that Benjamin Harrison found, on the high plateau of Kent, those primitive edge-flaked stones which are known as "eoliths." Long and fierce were the battles fought with a view to reaching a decision as to whether the flaking on these specimens is to be regarded as of human or natural origin; and these conflicts are not yet over. But they lessen in intensity, and it may be that the day is not far distant when the outstanding importance of Benjamin Harrison's discovery will be recognised by all students of early man. In this book one can read of Harrison's life, and gain an understanding of what manner of man he was, of the difficulties he met and overcame, and of yet one more example of the amateur's value to science. Sir Edward's volume should be acquired by all those who want to know something of one of the pioneers who penetrated deeply into the dimly-known country of man's remote past.

J. REID MOIR.

History:

Biggar.

*Marc Lescarbot. Nova Francia, a description of Acadia, 1606.* Translated by P. Erondelle, 1609. With an Introduction by H. P. Biggar, D.Litt. The Broadway Travellers, edited by Sir E. Denison Ross and Eileen Power. London: Routledge, 1928. Pp. xxxi + 345. 2 maps. 8½ in. × 5½ in.

Four years before the assassination of Henri Quatre, M. Lescarbot, a barrister of some distinction in Paris, accompanied the Seigneur de Poutrincourt to the new settlement of Port Royal in Acadia, on what is now called the Bay of Fundy. He stayed there for a year, and, after his return, in 1609, published his "Histoire de la Nouvelle France," parts of which—Book ii, chapters 31 to 48, and Book iii on the Manners and Customs of the Indians—were translated into English at the instance of Hakluyt. It is a reprint of this translation which forms a welcome addition to the Broadway Travellers' series.

Lescarbot was a well-read man with a fresh, observant mind and a strong bent towards natural philosophy. He had the makings of a field ethnologist, too; noted down the words of the medicine-men's songs, with the tunes in sol-fa; and did not miss the chance of seeing the Indians' war customs; though, as he says, having no intention at the time of writing a report, he took no note of many things he might have observed. His geography and anthropology were dominated by the reigning theory of climatic zones.

Lescarbot ekes out his first-hand observations on the Souriquois (Micmac of New Brunswick), their neighbours the Etechemins (Malecite) and their enemies the Armouchiquois of Saco River, Maine, with much comparative material from the classical authors, the Scriptures, Laudonnière's History of Florida, Hariot's Brief Report of Virginia, Jean de Léry on the Brazilians, and Acosta on Peru, with an occasional glance at French folklore (pp. 320, 203, 177). He arranges it on something like our modern scheme of the *life-history of the individual in society*:—

"Let us, therefore, begin," he writes, "with man from his birth, and having in gross marked out what the course of his life is, we will conduct him to the grave, there to leave him to rest and also to repose ourselves."

A brief résumé will show Lescarbot's anthropological turn of mind.

The chapters deal with Birth; Naming Customs; Rearing of Children (cradle-board, 152); Relations between Parents and Children; Religion (Souriquois have no idols; myth of men and women sprung from arrows; Souriquois attitude to Christian stories, 158; myths of interviews with a chief god; Armouchiquois, many *montoac* made by a chief god; a Virginian soul-journey, 165); *Soothsayers and Masters of Ceremonies* ("Aoutmoins"; the chief is also an Aoutmoin or has one attached to him, and the office is hereditary, 173, 178; Aoutmoin conjures with his head in a pit, 174; words and music of songs, 175, *Language* (numerals in four Canadian language phonetic peculiarities; Basque used in the trade jargon, 183). Clothing and Hairdressing. *Of the Form, Colour and Activity of the Savages, and why the Americans are not black* (effect of climatic zones modified by moisture, 194; Americans glabrous, 196). *Face Painting. Ornaments. Marriage* ("frigidum Venus" of the Americans; modesty independent of clothing, 217). *The Tabagie* or banquet: food; tobacco supplies the want of wine. *Dances and Songs. Physic and Surgery*: sweat-houses, sucking, scarifying. *Men's Occupations*, weapons, fishing, canoes and

dug-outs; agriculture and pottery of the Armouchiquois, disused among the Souriquois under influence of white traders, 247; houses; game of bowl-and-beans, 250, *Women's Occupations*: mat-making, sewing, basketry, leather and quill-work, bark dishes, canoes, tillage, 253. *Civility*: daily life, salutations. Virtues and Vices. *Hunting*: animals hunted, dress, use of dogs; cooking with red-hot stones in trunk of tree, 270. *Fowling. Fishing*: an important food-supply of the Souriquois; weirs. *Quality of the Soil*: Armouchiquois grow maize in hills with beans, use shellfish as fertilizer, 295; store maize in mat-lined pits, 296; grow hemp and a sort of cotton (?), tobacco; method of smoking; wild potato. *War*: Indians' wars not for possession of land but for glory or revenge, 308; Souriquois, mock siege of war-party by women, 308; not scalps but heads and dried skin of enemy taken, 168, 237, 309, 314; ritual of head-taker; shields, weapons and drill of war party, 312. *Funerals*: body dried and (?) embalmed, kept until war party starts to avenge death, deposited on scaffold on island, with offerings, 318-328; organised lamentations, 319; personal property of deceased burnt, 323. On p. 101 he notes either a nose-flute or panpipes played with the nose.

Lescarbot had great hopes of the future of the American Indians, and wrote his book partly in order that "their children might know hereafter what their fathers were, and bless them that have employed themselves in their conversion and reformation of their incivility." Not very many, unfortunately, are left to do so. The Massachusetts Indians have been swept away by wars, epidemics and dissipation, or survive as half-negro mongrels. "The single exception," says Mooney (*Aboriginal Population of America*, Smithsonian Miscellaneous Collections, Vol. 80, No. 7, 1928), "is the Abnaki tribe,"—including Lescarbot's Etechemins and Armouchiquois—"which still keep an independent existence with fairly healthy blood, owing to the watchful care of devoted missionaries": 3,000 in the year 1600, they numbered 1,400 in 1907. The Micmacs have possibly increased in numbers—from 3,500 to 4,500—but by the incorporation of mixed blood.

BARBARA AITKEN.

Africa: West.

Migeod.

*A View of Sierra Leone.* By Frederick William Hugh Migeod. 144 London: Kegan Paul, Trench, Trubner & Co., Ltd. 1926. Pp. x + 351. Illust. and map.

Mr. Migeod is an indefatigable traveller and a keen observer. The present book is

the outcome of six months' travel in Sierra Leone. Part I, occupying about two-thirds of the volume, is an account of the journey and is packed in a haphazard fashion with information of every kind concerning the conditions of travel, the country and its inhabitants, interspersed with anecdotes and personal experiences. Mr. Migeod notes everything with equal impartiality, including the local conditions of education, agriculture, geology, marriage and burial customs, and the number of chevrons worn by the police.

It is often difficult to understand Mr. Migeod's comments on the facts that he records; for instance, after telling of a human foundation sacrifice performed for the house of a paramount chief among the Temne, he states: "It is probable that this custom is not a very ancient one, and that it came in with the first contact with Mohammedanism. It is not a pagan negro custom, and is more likely of Semitic origin," but he quotes no evidence for this strange statement.

Part II is an account of the Mende people, and contains much of interest. Naturally, Mr. Migeod makes no claim to treat any subject exhaustively, but a more methodical arrangement of his material would have made Part II more valuable, whereas Part I is well-nigh unreadable through lack of any kind of arrangement except apparently the time order in which the author happened to make his observations. The best chapters are those on the Secret Societies, whose powers, though necessarily restricted by white influence, have by no means disappeared. It is interesting to know that farm work is regulated by the famous Poro society, the local Poro leader putting up his emblem indicating that work on the palm plantations must cease and preparations for the rice farms be made.

There is a chapter on dreams, another on proverbs, some Mende games are described, and a number of stories are related.

B. Z. S.

## CORRESPONDENCE.

Ireland: Archæology.

### The Sligo "Artifacts."

To the Editor of MAN.

SIR,—In view of the possibility of some of the readers of Professor Macalister's recent letter to MAN (September, 1928) being misled by the statements it contains, it is necessary to make the following observations:—

(1) Professor Macalister states that the verdict of geology against a palæolithic date for the "Sligo objects" is "absolute and insuperable." "This," we are told, "is the opinion of every competent geologist who has visited the place"; and, again, that "it is not too much to call it inconceivable that there should have been a palæolithic dwelling-site at any of the spots where the specimens have been collected." Since, however, Professor Macalister penned these sweeping statements, a report has been published (*Nature*, September, 1928) by Mr. Ernest Dixon, of H.M. Geological Survey, who visited the Rosses Point site with Mr. Burchell, and who has formed the opinion that the latter investigator's geological views on this place are correct. Thus, it will be realised that to claim that every competent geologist who has visited Rosses Point is in opposition to Mr. Burchell's opinions is untrue.

(2) It is by no means easy to ascertain what exactly are Professor Macalister's views upon the Sligo artifacts, for while

he states in his letter that "we might admit the possibility of their being artifacts of a later date," he also claims that he finds "it easier to believe that these amorphous objects have been formed by some natural force . . . than that any community of a stage later than palæolithic, fashioned them artificially." Finally, he says, "I ask myself if the relics of shore *débris* there illustrated (in the Sligo memoir) . . ." The confusion of thought shown by these two statements is clearly apparent, for how is it possible for Professor Macalister to admit the possibility of the Sligo specimens being artifacts, and yet to regard them as amorphous objects formed by some natural force? The only conclusion must be that, as I have suspected all along, he is lacking in the necessary knowledge to guide him in deciding whether a stone has been flaked by man or by Nature.

(3) I must protest against Professor Macalister's attempt to saddle me with some of his own regrettable practice of dogmatic assertion, in stating that I definitely claimed that the stones he picked up have been fractured by "thermal disruption." What I actually said was that these specimens had "apparently" been fractured by this means. If, however, he objects to the idea of the breaking of these stones by heat, I would suggest to him that their disruption was, perhaps, brought about by cold, of which the resulting frac-



tures are very similar to those produced by the former agent of fracture. What is the nature of the evidence which induces Professor Macalister to assume that the chunks of rock he collected have been "not long detached" from the solid rock, and why should he disregard the possibility of the specimens having been derived from the Boulder Clay which caps the limestone?

(4) I cannot allow to pass unchallenged Professor Macalister's insinuation that my attitude in regard to Mr. Burchell's views of the geology of the Rosses Point site is comparable with that taken up by him about the implements from this place. I have been meticulously careful to state that I have never seen the site of the discovery, and I have rigorously refrained from making any dogmatic statement upon it. Professor Macalister has, however, from the first, denied dogmatically, and unsupported by any evidence, the human origin of specimens he had not seen. This, in my experience, is unique in its complete disregard of elementary scientific caution and procedure.

(5) It may appear "enough" for Professor Macalister to close our memoir on the Sligo implements and to consign it to his topmost shelf where, as he tells us, he allows dust to accumulate. But such futile putting aside of inconvenient evidence will avail him nothing in his attempts to extricate himself from the totally impossible position into which he has fallen in this Sligo controversy. Rather, let him discuss, as I have done, with M. l'Abbé Breuil, who has excavated in the Grotte de l'Observatoire, the limestone implements there found, and ascertain from him what is his opinion upon the similarity of some of them to others found in Sligo.

(6) Lastly, while I note that Professor Charlesworth invites me, among other

archæologists, to visit the Rosses Point site (MAN, July, 1928), Professor Macalister advises me to do nothing of the kind, I am unable to grasp the meaning of the word "hogged," by which a Professor of Dublin University sees fit to describe the present condition of the area investigated by Mr. Burchell. This is surely a new addition to scientific terminology, but I am doubtful whether many people would look with favour upon its inclusion in archæological literature.

Yours faithfully,

J. REID MOIR.

G. JAMES.

GORDON.

### Wrestling.

To the Editor of MAN.

SIR,—Mr. Hornblower, in his 146 paper on wrestling (MAN, 1928, 43), seems to have missed the most important feature, which was quite likely omitted by the correspondent to *The Times* in his account.

When all the bouts are over, each competitor, successful and unsuccessful, goes up of his own accord and pays his respects to the arena by repeating the joining of the hands, the touching of the earth, and the lifting of it to the forehead. This shows that the ceremony is no empty formula without meaning to the combatants, and largely for the benefit of the spectators, but a true propitiatory act of respect to the presiding spirit of the arena.

This is emphasised by the fact that it is done without ostentation while the spectators are dispersing and nobody is paying any particular attention to them. In this way probably it has often escaped the notice of any onlooker who might later wish to record his observations.

Yours faithfully,

D. H. GORDON, Capt.

## ANTHROPOLOGICAL NOTES.

*Committee on Anthropological Research, Australia*: Report for the twelve months ending 31st July, 1928. The following report is published by the courtesy of Professor A. R. Radcliffe-Brown. 147

There have been four meetings of the committee, one in Melbourne, one in Hobart, and two in Sydney.

The investigations into the physiology of the Australian aborigines which were begun last year by the Department of Physiology of the University of Sydney have been continued. Mr. G. E. Phillips, with an assistant, has visited the aboriginal reserves at Kyogle (N.S.W.) and Barambah (Qld.) and has

carried out an extensive series of observations on the blood. Dr. Wardlaw and Mr. Horsley visited Kyogle in January and made observations on basal metabolism.

Miss Ursula McConnel (Brisbane and London), after completing six months' research amongst the natives of the Archer River, Cape York Peninsula, returned to Sydney in December. After writing up the results of her first season's work, she left Sydney on 20th May for a second season's work in the same district.

Mr. H. Hogbin (Sydney), after a preliminary investigation of the natives of Rennell Island, proceeded to Ongtong Java (Lord Howe Islands) and began a systematic study of the



language and culture of the inhabitants. After a short visit to Sydney, he has now returned to Ongtong Java to complete his investigations. Mr. Hogbin's research is part of a systematic plan for the investigation of outlying Polynesian peoples in the Melanesian area. The natives of Ongtong Java have decreased in numbers from 5,000 to less than 600 in thirty years. It is now too late to make direct observations on some aspects of their former culture, and only indirect information arrived only just in time to satisfy a great interest.

Mr. R. F. Fortune (New Zealand and Cambridge) left Sydney on 18th October for the New Hebrides Archipelago. He selected for his principal area the island of Tewara, but visited also Dobu, the Amphletts, the Trobriands and Fergusson Island. After a visit to Sydney, during which he wrote a preliminary account of his results, he has now returned to New Zealand. The results of his research will be published in due course.

Dr. W. G. Ivens (Melbourne) spent six months (June-December, 1927) in investigating the natives of the islands off the N.E. coast of Mala, Solomon Islands. He has proceeded to England for the purpose of writing up and publishing his results.

Mr. D. F. Thomson (Melbourne and Sydney) left Sydney on 21st April, and is engaged in a study of some of the native tribes of the Cape York Peninsula.

Mr. C. W. M. Hart (Sydney), holding a Science Research Scholarship of the University of Sydney, left for the Northern Territory on 3rd April. He is at work amongst the natives of Melville and Bathurst Islands.

Dr. Raymond Firth (New Zealand and London) arrived in Sydney on 1st May and left on 9th May for the island of Tikopia, where he will work for twelve months. The natives of Tikopia are another of those Polynesian-speaking peoples of Melanesia the systematic investigation of which, begun with Hogbin's visit to Rennell and Ongtong Java, it is planned to carry out before it is too late.

All the above field investigations have been financed from the funds for anthropological research provided by the Rockefeller Foundation.

Dr. A. P. Elkin (Sydney and London) was awarded a fellowship as from 1st September, 1927. He left Sydney in October for Western Australia, and has since been investigating the native tribes of the Kimberley District. Numerous reports have been received from him, and these show that he has succeeded in obtaining a great deal of important information chiefly about the social organisation of these tribes.

Dr. A. J. Canny (Sydney) has been appointed to a fellowship and left Sydney on 8th February for England, where he is carrying out research in physiology under the direction of Professor Hill.

Dr. W. L. Warner (California), whose expenses are met from the fund provided by

the Rockefeller Foundation for visiting investigators in anthropology, returned from his first expedition to the Northern Territory towards the end of 1927, and took up the task of comparing the data he had collected with those available for other parts of Australia. After working for some time in the Sydney Museum, he visited the Museums of Melbourne and Adelaide, and has prepared a valuable set of notes on the distribution of Australian culture-trait. His measurements of the natives of the Northern Territory have been dealt with statistically by Messrs. Bradfield and Spencer (Department of Physiology, University of Sydney). On 3rd April, Dr. Warner left Sydney for a further season's work in the Northern Territory. The results of his investigations up to date are of great importance for the understanding of Australian culture.

Under the new arrangement, by which the funds provided by the Rockefeller Foundation for visiting professors and special investigators are now administered by the Council, invitations to visit Australia have been sent to Mr. S. H. Ray (England), Dr. S. D. Porteus (Honolulu), Professor Earle (Shanghai), Professor Shellshear (Hong Kong) and Monsieur Leenhardt (Paris). Mr. Ray was unable to accept the invitation. Dr. Porteus and M. Leenhardt have accepted and will visit Australia in 1929. Professor Earle paid a short visit to Australia on his way from Shanghai to Europe. A conference was held at Sydney during Professor Earle's visit, and the subject of comparative racial physiology particularly with reference to basal metabolism was discussed at length. This conference and the visit of Professor Earle will have valuable results in co-ordinating the work in this field in different parts of Australia, and between Australia and China and possibly other countries. Professor Shellshear accepted the invitation of the Council, and is now in Sydney making a detailed study of the brain of the Australian aborigine in the light of the discoveries he has made in recent years as the result of his study of the Chinese brain. Professor Shellshear has obtained results of considerable importance which constitute a definite step towards a discrimination of racial types of cerebral morphology.

The reports of the various field-workers have made even more apparent than before the extreme urgency of the work for which the committee is responsible. Much that might have been done ten or even five years ago is now impossible for ever, and much that could be done now will be impossible five or ten years hence. The natives of Australia and of many parts of Melanesia are dying out with terrible rapidity, or are abandoning their habits and customs so rapidly that within a generation all memory of them will have been lost. The present resources of the committee (both with regard to funds and trained investigators) are insufficient to do more than a small part of what most urgently requires to be done.





FIG. 1.



FIG. 2.



FIGS. 1 AND 3.—HOMO.

FIGS. 2 AND 4.—CYPHANTHROPUS.

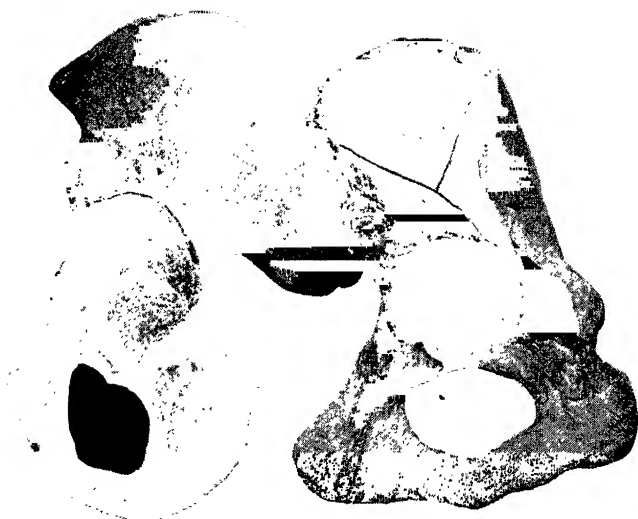


FIG. 5.—ACETÆBUTUM OF HOMO AND CYPHANTHROPUS.  
THE OS COXA OF HOMO AND CYPHANTHROPUS.





FIG. 1.—BANTU.



FIG. 2.—RHODESIA.



FIG. 3.—BANTU.



FIG. 4.—RHODESIA.



FIG. 5.—CABULESE.



FIG. 6.—CABULESE.

THE OS COXA OF HOMO AND CYPHANTHROPUS.

## ORIGINAL ARTICLES.

With Plates M, N.

Anthropology: Physical.

Pycraft.

**Some Suggestions for the Analysis of the Os Coxa in Man.** By **148**  
*W. P. Pycraft, Assistant Keeper, Zoological Department, British Museum*  
*(Natural History). With Plates M, N.*

So far the only records of the characteristic features of the human pelvic girdle are those which take the form of indices derived from the pelvis as a whole.

In the course of my study of the iliac fragment of Rhodesian Man I found it necessary to take a number of measurements, not hitherto taken, in order to find expression for the many and striking features which this fragment presented. These measurements will be found in my Report on the skeletal remains of Rhodesian Man recently published by the Trustees of the British Museum.\* But since this was written I have had occasion to pursue the subject of the analysis of the pelvis still further; and I venture now to publish the results as a nucleus for further work, for not until a very large number of pelves have been measured after this fashion shall we be able to say whether the peculiarities I have brought to light are due to age, sex, or race, or to a combination of these factors.

I shall also take the opportunity, now afforded me, to make a few additional comments on the Rhodesian pelvis occasioned by a more intensive study of this fragment which has followed the development of the analytical methods just referred to. These methods, it will be noted, are confined to the *os coxa* itself, and are intended to supplement the usual measurements and indices extracted from the pelvic girdle as a whole.

The photographs present the external and internal aspects of the *os coxa* in Rhodesian Man (restored) and of the Bantu, with which it was originally compared, as well as two others marked for measurement. The striking differences between them revealed by this method of comparison should go far to gain for my proposed scheme a fair hearing.

EXTERNAL ASPECT OF *Os Coxa*.

The analysis of the external aspect of the *os coxa* starts with a base-line drawn across from the anterior superior to the posterior inferior spine of the iliac crest. Its length is to be recorded as the "length of the iliac base-line." At right angles to this two other lines are to be drawn, (1) an anterior extending upwards to the crest of the ilium, and downwards to the anterior border of the acetabulum at the base of the anterior inferior iliac spine: this is the *acetabular line*; and (2) a posterior, from the point where the base-line cuts the summit of the sciatic notch to the crest of the ilium above: this is the *supra-sciatic line*. These two lines divide the ilium into three areas: an anterior, median, and posterior. The width of each is to be noted. The distance between the acetabulum and the base-line presents interesting differences when a number of pelves are being measured. This is the *supra-acetabular distance*. Next the ischial width is to be taken: this refers to the width of the ischium between the ischiadic notch on the one side and the posterior rim of the acetabulum on the other.

The relative size of the inferior anterior iliac spine calls for careful examination. The form and size of the acetabulum presents a greater range of variation than is generally suspected; and this is true also of the *facies lunatus*, and the acetabular fossa. The significance of these variations has yet to be explored.

Finally, careful note should be taken of the form and relative development of the ischial tuberosity.

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\* "Rhodesian Man and Associated Remains," 1928.

THE INTERNAL ASPECT OF THE *Os Coxa*.

For the analysis of the inner aspect of the *os innominatum* the ilio-pectineal line, or *linea arcuata*, is to be taken as a base line, and for convenience is to be orientated to form a horizontal plane. Its length should be recorded. At right angles to this a line is to be drawn from the anterior superior spine of the iliac crest and carried downward across the base-line. Commonly it terminates on the inferior ischiadic spine. This is the ilio-ischiadic line: and it brings out some striking structural features. The ischiadic spine, for example, may lie either directly on this line, or to one side of it. Another point to be noted is its relation to the anterior border of the *facies auricularis*—the articular surface for the sacrum; the distance therefrom is sometimes considerable. The measurement is to be taken from the point of intersection with the *linea arcuata*. It should be recorded as the pre-auricular length. Another line of importance is that drawn from the anterior border of the *facies auricularis* at right angles to the *linea arcuata*, and upwards to the crest of the ilium. This is the *supra-auricular line*, and should be recorded as the supra-auricular length. It affords a dividing line of considerable interest, for in some *os coxae* it may pass, for example, through the centre of the ilium, while in others it may be far distad of the centre of the iliac field, as to leave less than 30 mm. between itself and the *spina anterior superior*. The width of the plate on either side of this line should be recorded as the pre-iliac and post-iliac widths. The pre-iliac width is to be taken from the base of the anterior inferior spine; the post-iliac width from the base of the dividing line backwards across the auricle to the iliac crest.

The supra-auricular height may be taken along this line, for convenience, though sometimes the superior border of the *facies auricularis* rises somewhat above the level of the *linea arcuata*.

An important measurement, especially when comparing modern *os coxae* with those of extinct man, is the distance between the inferior border of the *facies auricularis* and the inferior border of the acetabulum - auriculo-acetabular length. In modern pelves this averages about 85 mm.—75 for pigmy races; in Rhodesian Man it probably did not exceed 50 mm.

The form of the *facies auricularis* should also be recorded. In some cases it is elongated and podiform; in others, semilunar. When the shape is podiform there is a long limb, and a short upper segment; in the semilunar type the segments are subequal.

Some of the more striking features brought out by the method of analysis now proposed are shown in the adjoining figures of the external and internal aspects of the *os coxa* of Rhodesian Man, the Bantu (1911.5.11.4) and a Cabulese (85.2.16.6).

Taking first the external aspect, it will be noticed that the pre-, median-, and posterior areas of the ilium show very striking differences of form. Thus in *Cyphanthropus* the pre-iliac area is of great size, its boundary-line rising upwards from the anterior border of the acetabulum, passes far behind the great obliquus lip on the iliac crest. Its anterior border from the base of the *spina anterior inferior* to the *spina anterior superior* is of great length. The median area is crossed at its base by a broad, and relatively steep ridge, from which the iliac surface falls away steeply on both sides. The posterior area is relatively small. The distance between the sciatic notch and the obliquus lip is relatively enormous.

In the Bantu the anterior iliac area is relatively small: the distance between the anterior superior, and the base of the anterior inferior spine is conspicuously less than in *Cyphanthropus* (10 mm.). The median iliac area is greater in the Bantu and the posterior area very markedly greater than in the Rhodesian.

The Cabulese presents a striking difference in the form of the anterior iliac area, the acetabular line, forming its hinder border, passing upwards to terminate

just canted of the triangular oblique lip of the iliac crest. The form of the posterior area, again, differs markedly from that of the two preceding areas. In this ilium, however, we find a slight ridge running down the ilium from the oblique crest, recalling that of *Cyphanthropus*, but much less developed, and with a different trend.

An *os coxa* of an Australian aborigine, used during this investigation, recalls that of the Cabulese, save that the acetabular line passes far behind instead of almost into the oblique lip of the iliac crest, and in that the posterior iliac area is relatively smaller.

The internal aspects of the *os coxa*, shown in the accompanying plates, demonstrate no less forcibly the usefulness of this system of comparison.

In the Rhodesian (Plate N) the pre-iliac area, traced backwards from the base of the anterior inferior spine, parallel with the base-line, agrees well with that of the Bantu, save that it is narrower at the base, being but 61 mm., as against 75 mm. The Cabulese has a base of no more than 57 mm.; the post-iliac area is 83 against 70 in the Bantu. The supra-auricular height is 102 mm. as against 89 in the Bantu and 90 mm. in the Cabulese.

It may be objected that the posterior area of this ilium is largely "restored": nevertheless, this restoration must be approximately correct since it is made up as to its postero-superior border to agree with the level of the sacrum. The pre-auricular length in the Rhodesian is 45 mm., 50 mm. in the Bantu, and 36 mm. in the Cabulese, and 25 mm. in the Australian aborigine already referred to.

The ilio-ischiadic line seems always to pass downwards to terminate on or near to the inferior ischiadic spine, save only in *Cyphanthropus*, of which more will be said later.

But the most striking and the most profoundly important difference is found in the auriculo-acetabular length, which in *Cyphanthropus* could not have exceeded 50 mm., whereas it is no less than 88 mm. in the Bantu, in the Cabulese 86, and the Australian 72 mm. This last was a small pelvis, having a maximum iliac width of no more than 141 mm., as against 158 in the Bantu and 157 in the Cabulese.

Those who are inclined to question the value of these suggested measurements should bear in mind that the study of physical anthropology is not limited to the skeletons of the genus *Homo*. We cannot hope to interpret aright the various and varying characteristics of the skeleton of existing races of Man without the aid of the records of the past.

#### THE PELVIS OF CYPHANTHROPUS.

In the Report on the remains of Rhodesian Man already referred to I described the *os coxa* of *Cyphanthropus* in considerable detail, pointing out that it differed profoundly from that of any other human pelvis so far known. There will be no need to repeat that description here, but one or two further, and quite important aspects of this fragment have come to light, as I have already remarked, in the course of working out the present system of analysis.

In that Report two figures of the iliac fragment are given, but they can scarcely be said to serve any useful purpose for those who will have perforce to read this Report apart from actual remains. Accordingly I give here photographs showing the external and internal aspects of my restoration, partly to remedy this defect, and partly to emphasise what I believe to be the value of this system of analysis for comparative purposes. Though restored, they show very clearly the shape of the actual bone, since this stands out sharply from the white, restored portions. For comparison therewith are shown the *os coxa* of the Bantu originally described in the Report referred to, a Cabulese, and an Australian aborigine.



The singular structural features of the pelvis of *Cyphanthropus* must be studied in conjunction with at least two of the five normæ of the skull, for the peculiarities of each show a reciprocal relationship.\*

The outstanding features of the *os coxa* of *Cyphanthropus* have just been recorded, but there remains something further to be said concerning the interpretation to be placed on its many singularities.

The ilium seen as a whole, from its external aspect and as restored, is conspicuous for its great height in proportion to its breadth. The actual fragment measures 130 mm. from the broken posterior inferior spine to the crest of the ilium at the obliquus lip—which stands over the ischial segment of the acetabular rim, the highest point of the ilium, as against 120 in the Bantu at the acetabular line. But to bring the posterior-inferior spine into its proper relation to the sacrum I have had to add 12 mm., making the *maximum* height 145 mm. as against 120. Take, again, in confirmation of the greater height, the distance from the top of the sciatic notch to the same point—the obliquus lip—on the iliac crest. This is 115 mm. as against 104 in the Bantu. The strongly concave iliac surface bounded by a great ridge running from an enormous obliquus lip on the iliac crest to the acetabular, and the backward position of this lip in regard to the *spina anterior superior*, has no parallel in modern pelvis. And no less remarkable is the enormous size of the oval *crista anterior inferior*, which is further marked by a peripheral groove, and a striking inward direction seen in no other pelvis. This crest it will be remembered serves for the attachment of the *rectus femoris*, and the ilio-femoral ligament. These features are, it is contended, responses to the stresses and strains brought about by the stooping posture when walking.

Without doubt, the many and striking singularities of the *os coxa* of *Cyphanthropus* are overshadowed in importance by the acetabulum. Concentrate attention on this and the key to the many unusual characters of the skull, sacrum, femur, and tibia is obtained. It affords indeed the crucial test as to the distinctness of this pelvis from that of *Homo*.

The attention is arrested by the first glance at the acetabulum, which is remarkable for its small size, the vertical and transverse diameters apparently did not exceed 50 mm., as against a vertical diameter of 62 mm. and a transverse of 58 mm. in the Bantu. No less striking is its extreme shallowness, having a depth of no more than 20 mm., as against 33 mm. in the Bantu, giving it the form of an inverted saucer looking downwards and forwards: and the curious form of the *facies lunatus*, that is to say of its glenoid surface.

It differs profoundly from that of *Homo*, wherein it takes the form of a deep cup, the sides of which form the glenoid surface, the acetabular fossa the bottom of the cup. The ischial segment of the glenoid surface in *Homo* sweeps downward and backwards, then sharply forwards to terminate at the acetabular notch immediately below the level of the *crista anterior inferior*. This is characteristic of *Homo*. In *Cyphanthropus*, on the contrary, the ischial segment of the glenoid surface has no forward sweep, but terminates at a point answering to a line drawn downwards from the obliquus lip of the *crista superior* and terminating at a point midway between the tip of the sciatic notch and the base of the *crista anterior inferior*. The area of the glenoid surface is reniform, with a very sharply defined raised edge to the hilum.

When we pass from a merely topographical description to anatomical tests, still more profound singularities emerge. If the ilium be held so that its anterior border is vertical and this border faces the observer, the acetabulum takes the form of an inverted saucer, tilted upwards to an angle of about 50°, and facing

\* I take this opportunity of repudiating the use of the word "norms" which my Editor preferred in that Report. The plural of norma is "normæ."

downwards and slightly forwards. But the ischial segment of its rim is swung backwards considerably behind the plane of its pubic segment. The glenoid surface forms a large, reniform area with a deep, sharply defined hilum. Regard the periphery of the acetabulum as the circumference of a wheel and fit this with an imaginary axle. It will be found that such a shaft, given a sufficient thrust, would pass backwards through the body of the ilium, parallel with the *linea arcuata*, to emerge within the *facies auricularis*.

Compare this with the acetabulum as seen in *Homo*, and orientated in the same way. Here it forms a deep cup. Treating its periphery as a wheel-rim, the axle would pass out in the centre of the triangular area bounded on the one side by the *linea arcuata* and on the other by the ischiadic notch: that is to say it pierces the wall of the acetabular fossa. In other words, in the one case this axis runs parallel with the plane of the ilium, in the other practically at right angles thereto.

The peculiar form and direction of the acetabulum of *Cyphanthropus* supports my contention that Rhodesian Man walked with a stoop, with the knees turned outwards. And this is further confirmed by the great size and inward twist of the anterior inferior spine, for the attachment of the rectus femoris and ilio-femoral ligament. The peculiarities of the femur and tibia, and the enormous width of the base of the skull, described in my Report on these remains, afford corroborative evidence in favour of this interpretation.

Since the methods of analysis herein described have served so forcibly to extract the differences between the pelvis of *Cyphanthropus* and that of *Homo*, they will probably serve a no less useful purpose when applied within this genus, not merely to express differences between individual *os coxa*, but probably racial differences as well.

Just as this paper was going to press my attention was called to a review of my Report in the *Museums Journal*, Vol. 28, p. 159, by Sir Arthur Keith. He contends therein that I have "come to a conclusion which will surprise all anatomists" who have had an opportunity of examining the original remains—namely that "Rhodesian Man progressed on limbs bent at knees and hips, with a forward stoop of body. The evidence in the reviewer's way of thinking indicates exactly the "opposite . . . ."

I venture to predict that when Sir Arthur Keith really does examine this fragment of the pelvis he will confirm my diagnosis.

#### COMPARATIVE TABLE OF MEASUREMENTS OF THE *Os Coxa*.

	<i>External aspect.</i>			
	Rhodesian.	Bantu.	Australian.	Cabulese.
1. Length iliac base-line - - - -	147	135	127	135
2. Acetabular line - - - -	133	119	105	112
3. Supra-sciatic line - - - -	100±	91	83	84
4. Supra-acetabular distance - - -	26	24	23	31
5. Anterior iliac width - - - -	53	44	46	47
6. Median - - - -	60	62	56	58
7. Posterior - - - -	45±	53	43	52
8. Ischial width - - - -	38-40±	40	32	41
	<i>Internal aspect.</i>			
	Rhodesian.	Bantu.	Australian.	Cabulese.
1. Ilio pectineal length - - - -	100±	117	100	123
2. Ilio ischiadic length - - - -	155±	147	143	152
3. Pre-auricular length - - - -	45	50	25	36
4. Supra - - - -	105	90	81	90
5. Pre-iliac width - - - -	55	75	50	57
6. Post-iliac width - - - -	83±	70	71	70
7. Auriculo-acetabular length - -	50	88	72	86
8. Maximum height, facies auricularis -	60±	64	51	56

W. P. PYCRAFT.

## Anthropology: Physical.

Clark.

**Rhodesian Man.** *By Professor W. E. Le Gros Clark.*

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In the report on Rhodesian Man recently issued by the British Museum (Natural History), Mr. Pycraft has given a description of the skeletal remains and, basing his evidence on these, has seen fit to create a new genus of Hominidæ—*Cyphanthropus*. There are a number of points in his description which call for criticism, but since the evidence of the pelvis has been so remarkably misinterpreted, and since this bone appears to be regarded as the most important indication for the creation of a new genus, I will confine my remarks to this part of the skeleton. The smaller pelvic fragment, E 720, which consists of a portion of the left ilium, Mr. Pycraft regards as belonging to the Rhodesian skull, though it may be remarked here that the evidence for this supposition is not convincing. I have had an opportunity of examining this fragment, for which I wish to express my thanks to the authorities of the Natural History Museum.

Let me first call attention to the accompanying diagram, which is a drawing of the acetabular region of a modern human male pelvis from the Department of

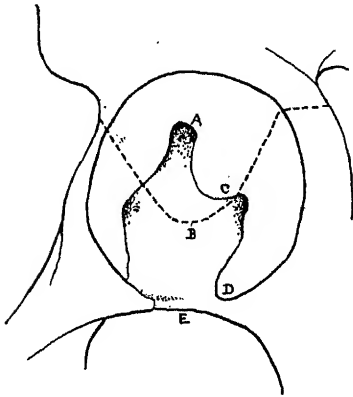


FIG. 1.—ACETABULAR REGION OF A MODERN HUMAN MALE PELVIS.

Anatomy, St. Bartholomew's Hospital Medical College. It is in no way an abnormal type of modern human pelvis, and it happens to illustrate very well the main points of the Rhodesian pelvic fragment. It will be seen from the diagram that, while the outer border of the acetabular articular surface is fairly evenly curved, this is by no means the case with the inner margin. At A, the inner margin forms a deep notch, directed upwards, in the centre of which are some large vascular foramina. Again, at C, the inner margin curves backwards and slightly upwards, turning down again rather abruptly to the postero-inferior extremity of the articular surface. This irregularity of the inner margin of the articular surface is to be noted in many modern pelvic bones, though it is rather more

distinct in this particular specimen than is generally the case. The other point to be noted here is that the floor of the acetabular cavity, at its centre, B, is often very thin, so that the bone here is found to be translucent when held up to the light.

A comparison of this specimen with the Rhodesian fragment shows perfectly clearly that the lower fractured margin of this fragment corresponds in position to the interrupted line shown in the diagram. In other words, to put the matter briefly, Mr. Pycraft has mistaken the lower margin of the iliac portion of the articular surface at C (which here curves upwards and slightly backwards) for the postero-inferior extremity of the entire articular surface at D, he has mistaken the lower broken margin of the fragment at B (which is really the broken thin floor of the centre of the cavity, and which can be seen quite distinctly in the actual specimen to be a broken edge) for the free lower border of the complete cavity at E, and he has mistaken the conspicuous notch A for the centre of the acetabulum. As a result of these errors, Mr. Pycraft has been led to reconstruct a pelvis with an acetabulum in which the ischial component is almost completely absent, and which thus bears no resemblance to any Primate (or, indeed, any terrestrial mammalian) acetabulum. The result is not only grotesque, but, to the eye of the mammalian anatomist, impossible. When these errors are realised and the acetabulum of the Rhodesian fragment reconstructed in a natural

way, the peculiarities of the cavity, upon which Mr. Pycraft lays such stress, disappear at once, and it is found, after all, to correspond quite well with the acetabulum of a modern human pelvis. Many of the peculiar measurements of the pelvic fragment will likewise assume more modern proportions. Thus, Mr. Pycraft says, "if the whole innominate be so held that the anterior superior and inferior spine are vertically above one another, and a horizontal line be drawn so as to pierce the centre of the acetabulum, then, in the Rhodesian, it will emerge through the *facies auricularis* just above the sciatic notch, whereas in the Bantu it will emerge some 45 mm. below the inferior border of that surface." If the acetabulum is correctly restored, however, and the correct centre of the cavity ascertained, this contrast disappears. Again, Mr. Pycraft says: "the distance between the inferior border of the auricular surface and the inferior border of the acetabulum differs sharply from that obtaining in any modern pelvis; and measures 55 mm. in the Rhodesian against in the Bantu 86 mm." But, as I have indicated above, he has mistaken a broken edge from the centre of the acetabulum for the inferior border. Thus is this discrepancy explained. It seems that Mr. Pycraft has been further misled by an incorrect orientation of the pelvis. Thus he makes the remarkable statement that "when the pelvis is orientated as in life with the body erect, the anterior inferior spine of the ilium is over the middle of the upper segment of the horse-shoe-shaped acetabular border." This, of course, is quite incorrect. Finally, there are some statements which simply seem to indicate the absence of sufficient material for comparative purposes. Mr. Pycraft states that the distance "from the highest point of the crest of the ilium to the horizontal *linea ilipectinea*" is 109 mm. in the Rhodesian fragment, and adds, "this exceptional height of the ilium is further associated with an increase of surface found in no other known human pelvis." Now, it is doubtful what is precisely meant by the "highest point of the crest," for what would be the highest point when the pelvic bone is in the position which it would occupy in the erect posture is not preserved in the Rhodesian fragment. But, supposing it to refer to a point on the iliac crest immediately behind the tubercle of the crest, then these measurements are not in any way exceptional. For, in the pelvis from which the diagram here figured was produced, and which was picked out from the first few pelvic bones examined in this department, the corresponding distance is at least equal to that of the Rhodesian fragment, while the area of the corresponding surface of the ilium is, if anything, rather greater. To say, therefore, that the ilium of Rhodesian man shows an increase of surface found in no other known human pelvis is hardly justifiable.

In general, therefore, it may be definitely stated that the Rhodesian pelvic fragment (such as it is) appears to represent a portion of an os innominatum essentially of modern human type. The great sciatic notch is a little lower than usual in relation to the acetabulum (though not so low as Mr. Pycraft imagines), and the concavity of the posterior portion of the dorsum ilii is rather more conspicuous than usual, but neither of these characters has been demonstrated to be even unique in degree.

When these curious errors are rectified, it will be seen that, according to the diagnosis given by Mr. Pycraft on page 49 of his monograph, the genus *Cyphanthropus* depends entirely on certain features of the skull. I find it impossible to believe that a comparison between the Rhodesian skull and the skulls of Neanderthal man will justify the creation of a separate genus for the former.

W. E. LE GROS CLARK.

Italy: Archæology.

Randall-MacIver.

**Chronology of the Early Iron Age in Italy.** . By D. Randall-MacIver, M.A., D.Sc.**150**

There is a very close measure of agreement among all leading archæologists as to the *relative* chronology of the Iron Age in Italy—that is to say, the sequence of the respective sites and their relationship. In regard to *absolute* chronology, on the other hand, there are wide divergencies of opinion. Broadly speaking, there are two schools. Those who share the opinions of Menghin and myself place the beginning of the First Benacci period at 1000 B.C. or earlier; while those who follow Ducati bring it down to the ninth, or even the eighth century. Each side musters numerous adherents, and healthy criticism is busy trying to find criteria for judging between them.

Professor Sundwall, whose Memoir I reviewed in the last number of MAN, is not precisely an adherent of either school. He produces reasons for refusing in the main to accept the longer dating, but differs from Ducati in placing the beginning of the Arnoaldi at 675 B.C., which is not so far from the 700 or 750 B.C. suggested by Menghin and myself. The beginning of the Bologna periods he places at 850 B.C. and, most audaciously, he reduces the entire Second Benacci to only fifty years, viz., 725–675 B.C. In disagreement with most Italian writers, he makes not only Pianello but also Terni and even Allumiere later than the beginning of the Bolognese, which obliges him to date them not earlier than the ninth century.

There are the strongest possible archæological objections to this arrangement, which is so revolutionary that it would need very cogent arguments to make it in any way acceptable. It is evident, for instance, that it would leave an immense gap unfilled between the end of the Bronze Age and the beginning of the Iron Age in Italy, which is quite improbable in view of the extent of the explorations already accomplished. And it contradicts the typological evidence of the fibulæ, which shows a graded series, closely linked at one end to the Bronze Age, passing by gradual stages into the Benacci periods of Bologna. We must enquire, therefore, what are the main arguments on which Sundwall bases his theory.

The most important of his axioms are the two following :—

- (1) The Villanovans were driven from their homes in Central Europe by a climatic change which took place in the ninth century. We are almost surprised that he does not state the exact year and month.
- (2) No painted pottery of Greek origin could come into Italy except *via* Cumæ, and therefore any sites on which such pottery occurs must be later than 750 B.C.

Each of these axioms I shall show to lack all logical foundation.

First of all as to the climatic change. It is a recent and rather favourite theory of some geographers that a deterioration of climate in Central Europe set in about the beginning of the first millennium B.C. I am not aware that the geographers have agreed, or could possibly agree, upon any date so exact as the middle of the ninth century. Climatic changes are not like eclipses, for which a precise day and hour can be fixed. There are probably competent geographers who would assign a date a hundred or a hundred and fifty or two hundred years earlier than that selected by Sundwall. But it is unnecessary to appeal to them. For I have yet to learn that a man is obliged to remain in an uncomfortable house until he is literally kicked out of it. A modern analogy will make my meaning clear. A few years ago the inhabitants of one of the islands off the west coast of Ross-shire were reduced to the verge of starvation by a succession of terrible summers which entailed the failure of every crop. Public subscriptions were raised and the unfortunate crofters were all enabled to emigrate to Canada. But it

needs only the very slightest knowledge of the Hebrides to enable us to assert that this was not the first instance of failing crops, and conditions that rendered life so hard as to be almost intolerable. Long before 1921 many a hard-pressed Highlander had emigrated, so that any Sundwall who should argue from this actually recorded migration that the first colonisation of Canada by Scotchmen began in that year would be wrong by a matter of several centuries. Similarly, the climatic change in Central Europe may have reached its final and most disastrous stage in 850 B.C., 950 B.C., or any other date that it may please the experts to assign; but there must have been ample warning of increasing hardship long before the final phase. Any prudent Villanovan may well have taken notice and moved from his inhospitable country without waiting to be compelled by sheer starvation.

I have chosen to meet Sundwall's argument with the same seriousness that he attaches to it, but I must point out that his whole assumption of the motive for migration is entirely gratuitous. The real motive may equally well have been any one out of the many which are known to have actually operated in the recorded history of one country or another. It might have been a change of dynasty, a quarrel between chiefs, the pressure of neighbouring tribes (not necessarily economic), superstitious fears or oracles, pestilence among men or beasts, or even the shortage of some favourite but not indispensable foodstuff.

In a word Sundwall's first axiom has no logical value whatsoever, and the whole question of the migration-date is left exactly where he found it. To proceed to the second axiom. This would have been accepted as perfectly sound only a few years ago. But it has been entirely disproved by Orsi's recent discoveries in Calabria. The Greek pottery of Canale (*see* my "Iron Age in Italy," Chapter 7 and Plate 41) belongs to the eighth and ninth centuries. It can easily be distinguished from the characteristic pottery used at Cumæ about the end of the eighth century (for which *see* my "Iron Age in Italy," Plates 35, 36). In fact, it belongs to an earlier phase, directly connected with the Dipylon schools. The really logical process, therefore, is to reverse Sundwall's reasoning, and maintain that the presence of certain classes of geometric pottery on any site proves that site to be at least as early as 750 B.C., and very possibly earlier. Thus I obtain complete confirmation of my tentative dating for the geometric pottery on the Sopra-Selciatello site at Corneto ("Villanovans and Early Etruscans," pp. 47-48 and Plate 11). It resembles the Greek pottery of Canale and belongs therefore to the eighth, or even possibly to the ninth, century; and, of course, it did not come through Cumæ.

Sundwall's innovations, then, are unnecessary and unconvincing. Nevertheless, we must be grateful to him for raising these possible points of discussion, and especially for casting his argument into a clear and systematic form. It would greatly assist progress if Ducati would come out with an equally clear-cut and closely-reasoned exposition. At present his opinions are given only in the form of *pronunciamentos*, chiefly contained in such brilliant semi-popular works as "Etruria Antica" and "L'Arte Etrusca." These do not lend themselves to that close debate and discussion which is the only method of arriving at convincing conclusions.

From what he has written, however, it is evident that the divergencies between Ducati and myself—at least in regard to Etruria—may be reduced to very small proportions after the middle of the eighth century. The general student might well adopt a compromise between the two, recognising that there are strong arguments on each side, and that in any case it is virtually impossible to date any tomb more closely than within the limits of a generation. I may clear the way by two concessions, due to the criticisms of Karo and Ducati, which I recognise as well-founded.

First, then, the Warrior's Tomb of Corneto is a serious crux. We are all agreed as to its relative place. It is the earliest of the definitely Palæo-Etruscan tombs. My proposed dating of it to 850 or 800 B.C. has been generally rejected. Nevertheless, the positively Villanovan character of many of the objects affiliates this tomb closely to the Second Benacci. I will abandon, therefore, the date of 850 B.C. as scarcely tenable, but will still maintain that it is hardly reasonable to place the warrior's tomb later than the first half of the eighth century.

Secondly, I did not fully appreciate when writing "Villanovans and Early Etruscans," that some of the Vetulonian tombs which I was quite willing to put into the early seventh century do undoubtedly come down as low as 650 B.C. These are the Migliarine tombs, and the external graves of the Pietrera mound. The Tomb of the Lictor is also seventh century, probably about 670 B.C.

Apart from these modifications, I adhere to the dating given in that volume, which, although tentative, seems to stand the test of examination. One of the most important landmarks for Palæo-Etruscan chronology is the Tomb of Bocchoris, and the attempts to explain away its evidence are merely unscientific. The vase inscribed with the name of Bocchoris was manufactured between 734 and 728 B.C. Consequently, the date of the tomb may quite possibly be as early as 730 B.C., and though I do not maintain this, I insist that the possibility must be remembered. For the ineradicable tendency of a whole school of writers to exclude all but the minimum antiquity in dating a tomb is a mere prejudice, and should be recognised as such. On the other hand, it is fair, as Montelius always maintained, to remember the possibility that an object may have been acquired by a quite young man and buried only when he died in extreme old age. The probabilities, of course, lie between the two extremes. The maximum degree of elasticity allowable would bring the Bocchoris tomb down to 680 B.C. Ducati chooses the date of 680 B.C.; it is logically possible, but this is extreme partisanship. I do not even pause to discuss the untenable hypothesis that a valuable object was passed on to later generations; Ducati shows his good judgment in disregarding it. Viewing all the circumstances, it seems to me that the student will be wise to assign the Bocchoris tomb to 700 B.C. with a possible variation of 20 years plus or minus.

On the same principle of striking a mean between Ducati's dates and my own, which would not be entirely unacceptable to either of us, I would propose the following working scheme, allowing a variation of plus and minus 20 years in each case, which will actually meet the variations of opinion hitherto expressed. At the lowest end the *Grotto of Isis*, 600 B.C.  $\pm$  20 years; *Regolini-Galassi*, 640 B.C.  $\pm$  20 years; *Barberini*, 650 B.C.  $\pm$  20 years; *Bernardini*, 660 B.C.  $\pm$  20 years; *Migliarine*, 660 B.C.  $\pm$  20 years; *Tomb of Lictor*, 670 B.C.  $\pm$  20 years. The Tomba del Duce seems to be distinctly more ancient than the Tomb of the Lictor; its date is probably near 700 B.C. The oldest Etruscan tombs of Vetulonia are distinctly eighth century and not seventh, but how far back into that century they should be taken is still a matter for discussion. To this part of the subject I hope to return within a few months, and in the meanwhile shall hope to focus any useful criticism.

D. RANDALL-MACIVER.

## REVIEWS.

Assyria: Religion. Campbell-Thompson.

*The Epic of Gilgamesh, rendered literally into English Hexameters.*

By R. Campbell Thompson. London: Luzac & Co., 1928.

This is a remarkable achievement, demanding a combination of Assyriological scholarship and English literary ability

which it would be difficult to match. Dr. Campbell Thompson has performed his task with extraordinary skill and ingenuity, with the result that such portions of the old Babylonian Epic of Gilgames, or, as Dr. Thompson writes the name, Gilgamesh, as survive are now presented to the English reader in a worthy form.

The existence of the Epic is now fairly well known to the British public, thanks mainly to the fact that the eleventh book of it contains what its discoverer, Mr. George Smith, called "the Chaldean account of the Deluge." The work consisted of twelve books and included some of the chief legends of Babylonia going back to pre-Semitic times. The leading figure in it, around whom the legends were clustered, was a famous king of early Erech, who had built the great walls of the city and obtained name and fame for himself by the conquest of his neighbours. The Epic in its later Assyrian form was the work of a certain Sin-ligi-unnini, but we have portions of earlier Babylonian texts which go back to the age of Kham-murabi (B.C. 2000), if not earlier, as well as fragments of translations into Hittite and Mitannian which have been discovered at Boghaz-keui. These latter show what a vogue the Epic must have had in the ancient Oriental world.

The story revolves round four chief scenes in the life of its hero. In the first he is brought into contact with Enkidu, the personification of the uncivilised savages who lived in the forests and hills that surrounded Babylonia and even in Babylonia itself in pre-Sumerian days, and who herded with the wild beasts, from whom they could scarcely be distinguished. But Enkidu was enticed from his primitive innocence and ignorance by the wiles of a *hetæra* and the seductions of the city; he left his Eden and entered Erech, where after a struggle with Gilgames he submitted to the superior prowess of the latter and became his servant and friend. Then follows the first result of the encounter: the expedition of the two heroes against the demon Khumbaba, the lord of the Cedar-forest, whose life was bound up with that of the cedar tree. But the cedar was cut down and its timber transported for the service of civilised life, while its master himself perished. As the Cedar-forest was identified with north-western Syria, it was this part of the Epic which had a special attraction for its Hittite and Mitannian admirers.

Divine revenge for the death of Khumbaba led to the second scene of the Poem, the fight with "the bull" sent by Anu, the god of heaven, against Erech. Once more the two heroes were successful, and the bull which had defied the onslaught of hundreds of armed men was slain and its spoils divided. But its death brought the goddess Istar on the stage. She made love to the hero under whose prowess the bull had fallen. Gilgames, however, rejected her suit with insults, and enumerated the many lovers whom she had brought to an

untimely end. The love of the goddess was turned into hatred, and divine vengeance was not slow in arriving. First, Enkidu sickened and died, and then Gilgames himself was smitten with a sore disease. In the vain hope of discovering a cure, he made his way across the western desert to the gates of Sunset, which were guarded by the "Scorpion-men." Beyond them lay the ocean which encircles this world and separates it from the land beyond the grave. With the help of the Sun-god, Gilgames was ferried across it by the Babylonian Charon and finally reached the Happy Islands, where Utnapistim, the Babylonian Noah, enjoys immortal existence. There, in answer to the question how he might obtain immortality for himself, Utnapistim described the Deluge and his subsequent translation into Paradise. Gilgames was then told to sink to the bottom of the ocean and pluck from it the plant which would bestow upon him the gift of eternal youth. He did accordingly, but on his way back to the earth, while resting by a well, a serpent stole the plant from him, and the hope of immortality was lost. The poem ends with the hero's dirge over the death of Enkidu and the hopeless fate of man. Even though two-thirds divine like himself, man cannot escape death and that dark underworld where all that makes life enjoyable is gone for ever.

The legends and beliefs embodied in the Poem are full of contact with those of other peoples, more especially in the Near East, and have consequently been a subject of considerable attention. A large amount of wild and unprofitable speculation has been expended upon them, particularly in Germany, as well as a good deal of sober and scholarly work. Immediately after Mr. George Smith's announcement of his discovery, Sir Henry Rawlinson suggested that the Epic was constructed on an astronomical basis, the subject-matter of each of its twelve books corresponding with a sign of the Zodiac; the suggestion was widely adopted, but subsequent discovery has shown that it was only partially, if at all, correct. In only a few instances does the subject of a book correspond with what, according to the theory, would be the equivalent Zodiacal sign.

But it must be remembered that a large portion of the Poem has yet to be recovered and that the gaps and doubtful readings in what we possess of it makes the interpretation of many passages uncertain. Dr. Campbell Thompson's fresh collation of the texts and literary ingenuity have cleared up several of them, but many still remain doubtful. At the very outset I should question, for instance, his view



that the tyranny of Gilgames was resisted by the citizens of Erech; it seems to me that it is not his fellow citizens or subjects who are referred to in the mutilated passage of the Poem, but the members of the neighbouring states whom he had compelled to serve him.

The origin of the Gilgames legends is still obscure. All we can say is that they go back to a prehistoric date. The figures of Gilgames and Enkidu are found on early seals, and the name of Gilgames, like his dress, does not seem to be Sumerian. But we are still ignorant as regards the pre-Sumerian period of Babylonia when the cursive cuneiform had not yet developed out of the primitive pictographs and the Bronze Age had but just begun.

A. H. SAYCE.

#### East Africa: Ethnography. Gutmann.

*Das Recht der Dschagga.* Von Bruno Gutmann, München: C. H. Beck'sche Verlagsbuchhandlung. 1926. Pp. 778. **152**

Dr. Gutmann is well known to anthropologists for his valuable researches among the Wachaga, in whose country he lived as a missionary for twenty years before the war. It is to the credit of the British Government that after the hostilities came to an end he was invited to return among the people he knows so well. Many of his writings are scattered through German periodicals, and in his "Dichten und Denken der Dschagga-Neger" he has produced, in a popular form which detracts nothing of its value, a survey of Chaga beliefs and traditions. In the present "Chaga Law" he has given to the world his *magnum opus*, a study of their tribal customs which, in the whole literature on Africa, may be equalled by two or three, but is certainly excelled by none. In clear language he describes every aspect of Chaga common law; though its smallest particulars are dealt with, the broad outlook is never obscured, as is frequently the case in modern books. He states his facts and then, with deep sympathy, profound erudition and a remarkable insight into the working of the natives' mind, he expounds the origin and evolution in the course of ages. The ethnographer, the ethnologist, the student of jurisprudence, and the psychologist will find in this book an abundant source of information and ample food for thought, as well as suggestions for the solution of many a vexed problem.

It is impossible to give a worthy review of this monumental work; few are those who are qualified to do so, and the most cursory survey would require space which exceeds anything that could be allotted to the reviewer. The book is crammed with

new facts and interesting suggestions; there is so much to be praised and so little to criticise. The only omission one has to record with deep regret is the lack of an index to a work which will serve as a book of reference to all those interested in the African in particular, and primitive man in general.

Our deepest gratitude is due to Dr. Gutmann for his invaluable contribution to our knowledge which, no doubt, will be cherished by posterity as a monument to a phase of African life which is fast passing away.

E. TORDAY.

#### Africa, South: Ethnography. Bleek.

*The Naron: a Bushman Tribe of the Central Kalahari.* By D. F. Bleek. (University of Cape Town: Publications of the School of African Life and Languages.) Cambridge: 1928. Pp. ix, 111. Price 10s. **153**

This little monograph, the first of a series of short studies of South African tribes projected by the University of Cape Town, is an important contribution to our knowledge of Bushman culture. It describes lucidly and concisely the social life, economics, religious beliefs and practices of the Naron, and also contains short grammatical sketches of the Naron and //k'au//en languages, as well as several folk-tales, with literal translations. The accounts of the puberty ceremonies for boys and girls and of the religious beliefs are especially valuable, and convey a great deal of new and interesting information; on the other hand, little is said about social organisation, an aspect of Bushman life which badly needs detailed investigation. Miss Bleek, who has already done invaluable work on the Bushman languages, has added greatly to the debt of students of the Bushmen by giving them in this monograph what, despite its deficiencies, may justifiably be regarded as the most complete account yet published of any Bushman tribe.

I. S.

#### Egypt: Linguistics. Gardiner.

*Egyptian Grammar.* By Alan H. Gardiner. Pp. xxviii, 595. 1927. **154**  
Oxford: Clarendon Press. Price 42s.

When Dr. Gardiner announced some years before the war that he was writing a grammar of the Egyptian language, all Egyptologists were on the tiptoe of expectation. They remained in that uncomfortable position until a few months ago, when this monumental work appeared; but it was worth waiting for. Dr. Gardiner's unique knowledge of the language and literature has made it possible for him to write a grammar which necessarily surpasses all others yet published. The book

begins with an introduction, comprising a sketch of the writing, language, and literature, and then comes the grammar proper, with appropriate exercises for translation of Egyptian into English and English into Egyptian. The appendices include the vocalisation of Middle Egyptian, the transcription of proper names, a list of hieroglyphic signs with their meanings, and a vocabulary. In this comprehensive work there is practically no aspect of the Egyptian language which is left untouched, though the book professes to deal exclusively with Middle Egyptian. A spécial fount of type has been used, made in the first instance for this work, but now available for other publications in which hieroglyphic signs are required. The type is based on signs of the eighteenth dynasty, not that this is the best period for the script, for the Old and Middle Kingdoms are finer in that respect, but the signs of the eighteenth dynasty are very numerous and are admirably suited for the purpose required. It is an important matter that the type is of a size suitable for printing with ordinary roman, and is, consequently, less cumbersome than the older founts now in use.

The main part of the book is divided into thirty-three "lessons," beginning with the direction of the writing, the phonograms, the alphabet, and other elementary matters, and ending with a "lesson" which includes types of sentences, various kinds of questions (this being again subdivided into seven sections), multiple sentences, ellipses, remarks on word-order, and concord. Throughout the book each point of grammar is lucidly explained and is illustrated by examples from Egyptian writings; the accompanying exercises being, of course, on the lessons to which they belong.

Besides the actual grammar the book contains an immense amount of information on allied subjects—information which is often difficult for a beginner to find.

Take, for example, Lesson XX on the numbers. This contains the method of writing and the use of cardinals and ordinals; then comes the method of expressing ordinary fractions. A most important section follows, in which the method of expressing fractions of weights and measures is explained. It is a good example of Dr. Gardiner's lucid exposition, in which all the facts are collected and set out in a clear and scholarly manner.

There are two great faults in the book. The first—which jumps to the eye—is its size. The present fashion in books on Egyptology is to make them as large as possible, and this book is in the fashion. Even the most enthusiastic student could not carry so ponderous a volume to read at odd times. It is essentially a book to lie on the writing-table to be consulted with due solemnity like a dictionary. The other fault—and this is more serious—is the arrangement of the material. The division into "lessons" is not a happy one, especially in the earlier part. In his anxiety to push the beginner on, Dr. Gardiner is apt to confuse him. Thus, instead of keeping together all the instruction on the writing, such as the biliteral signs, ideograms, determinatives and so on, it is spread over eight lessons intermixed with purely grammatical matter. In the same way the rules of grammar are not kept together. This applies to the earlier "lessons"; in the later "lessons," where the verb is fully treated, the matter and manner of presentation are extraordinarily fine. But it is the beginners who matter in every subject, and Dr. Gardiner's method is one which can be followed only by advanced students.

These two faults are, however, all that can be said against a very fine production, for it is a book which has brought our knowledge of Egyptian grammar to a point as exact and accurate as that of any other ancient language. M. A. MURRAY.

## CORRESPONDENCE.

Melanesia: Population. Pitt-Rivers.  
**Depopulation in Melanesia.**

To the Editor of MAN.

To the increasing number of people who are convinced of the urgency and importance of studying the facts and the causes of depopulation in the Pacific and elsewhere, it is gratifying to find that this subject is at last being seriously taken up. We are bound to welcome for this reason such investigations as those conducted by Mr. Baker which form the subject of his paper on "Depopulation in the New Hebrides" in the last number of the

*J.R.A.I.* (Vol. XVIII, 1928). It seems sufficiently important to examine his arguments in relation to the whole problem. It is the last of a fairly long list of inquiries into the causes of depopulation of particular areas. Yet, in spite of the confidence with which diagnosis and prognosis are pronounced, and the equally confident opinions of previous diagnosticians are set aside, we seem to be as far off as ever from a general principle and method of diagnosis, whereby we could possibly reconcile the causes of depopulation in this particular area, if correctly stated, with

the causes that could have produced the same results in many other areas peopled by very similar and related races.

It is, however, I think, possible to show, if certain obstinate diagnostic fallacies are discarded, that there is a common principle by which the problem of depopulation can be approached.

Briefly stated, races decline and die out, as do all living things, when environmental changes render them maladapted. For maladaptation to show itself in decline in population means, of course, that the death rate gradually overtakes and exceeds the birth rate, and the death rate, however categorised, represents the total factors of elimination. It is usually thought that by enumerating selected factors of elimination the decline is explained; that, by inference, the factors of elimination are the causes of maladaptation. This can be easily disproved by showing that there is often no correlation between increased elimination and decline; that is to say, an increased death-rate is often correlated with a still greater increase in birth rate and a lower mortality-rate with a declining population. Compare, for instance, the rates of the United States Indians (low mortality) with the negro population (high mortality).

Mr. Baker follows the usual approach to the problem. He enumerates various factors of elimination selected by himself and other investigators, marking the factors they prefer to lay stress upon and giving reasons for his own different selection. The result reads like the comparative marking of examination papers. Baker examines: endemic diseases, 1 point; introduced diseases, 3 points; abortion, 3 points; high sex-ratio, 2 points; apathy due to loss of old customs, etc., nil. W. H. R. Rivers examines: endemic diseases, nil; introduced diseases, 2 points; abortion, 2 points; high sex-ratio, nil; apathy due to loss of old customs, 3 points, and so on. The marking is quite arbitrary, and we are no nearer an answer to the questions: What has produced maladaptation? Why can fertility, always so far behind fecundity or possible fertility, no longer keep level with, or gain on, the factors of elimination?

Mr. Baker's answer appears to be as follows. Introduced diseases cause an appalling number of deaths. Therefore, "people think it useless to produce children, who will only die in epidemics." Therefore, they procure abortion, "usually" on instructions from the husbands, by "drinking infusions of the leaves of *Dracaena* and other plants." This systematic birth-control is the cause of the small families in Santo; the people in

the Christian villages do not feel quite so hopeless, and therefore, by procuring abortion rather less often, have rather larger families, and are dying out rather less fast. It would seem as though the dispensing druggist of the New Hebrides prescribes on husbands' instructions in quantities that can be exactly measured by the subtle variations in hopelessness that exist in the different districts, and these correspond to the different fertility rates. The chief evidence that the natives wish to limit their children in this way is supplied by the evidence of "a native woman on another island," who replied to a question about recent deaths in her village: "'Close up all piccaninny here 'e die finish' " (all these children will die soon)." It is implied she wished to forestall the inevitable.

Although, according to this theory, deliberate abortion caused by variant degrees of hopelessness, or fear of disease in conjunction with the two other factors mentioned—disease itself and high sex-ratio—determine the exact variation in the curve of population, Mr. Baker will have nothing to do with those psychological causes that Rivers, recently supported by Roberts, laid chief stress upon.

But if we are dealing in fact with variations in adaptation—that universal problem of organic life—the mechanism of it is both psychological and physiological. One is reflected in the other. In so far as psychological factors may be supposed to affect the direct factors of elimination, they may do so by bringing about a lowered resistance to the attacks of disease and by affecting fertility and potency. Physical degeneracy may be either a process of extinction or a substitution of one anthropological type for another (differential survival); in both cases there is ethnical disequilibrium. Ethnical disequilibrium may result from variation of some aspects of the cultural complex followed by variation in the whole complex, entailing a period of cultural change. Shirokogoroff reduces the process to a mathematical formula.

I find it particularly interesting that Mr. Baker attaches so much importance to high sex-ratio. Although I find it equally important, his interpretation is entirely different. He does not appear to realise that sex-ratio can vary progressively in the same group, and consequently does not allude to the significance of variation. He apparently makes the faulty assumption that sex ratios are constant for each ethnic group or population. If high sex-ratio is a cause of decline, and populations with a high sex-ratio had always had the same

ratio, how could any have survived to have been observed at all?

Mr. Baker's explanation of the significance and operation of different sex-ratios is ingenious though misleading, for he illustrates his theory by imagining three separate equal populations starting with the same numbers of men and women, but each population reproducing in different sex ratios of 90, 100 and 160 (males to 100 females). To these populations he ascribes no differential sex-survival rate, therefore only a differential birth sex-ratio, both sexes having an equal average span of life. (But it is invariably a differential sex survival rate which determines a high or low adult sex-ratio. The adult sex-ratio is often low when the birth sex-ratio is high.) In his hypothetical low sex-ratio population (90), polygyny must be practised so that every woman can bear children. The average child-bearing age of both sexes is the same. He shows by curves that under these conditions the population with high sex-ratio dwindles, the even ratio population remains unchanged, and the low one increases. All that this illustration shows is that in a population in which at every generation a diminishing proportion of females are born and survive, even if the average family per bearing female is three, the population must necessarily dwindle. More briefly, if at every generation under these conditions there are fewer future mothers born, there must be fewer children born. This proposition may be evident without mathematical demonstration. But nothing resembling the hypothetical conditions has ever occurred in any population, and it does not show how progressive masculinity can occur, as, actually, it does, in declining populations. For instance, to give an example: the New South Wales aboriginal population in the 1911-1921 decennial period declined from 2,012 to 1,501 and masculinity increased from 14.51 to 17.12 (excess males over females per 100 of both sexes): the Victorian aboriginals declined in the same period from 196 to 111, while the masculinity rose from 5.10 to 11.71. It is only in stable or increasing populations that the sex-ratio remains relatively constant.

Furthermore, as I attempted to show, the extent to which polygyny is numerically possible depends upon the variation in the nuptial ages, or the extent of the average male postponement in the nuptial age in conjunction with the adult sex-ratio, which I have called the effective mating sex-ratio, *i.e.*, the ratio of males to females at the respective nuptial ages. The postponed mating of the nubile males tends to counteract high sex-ratio.

Mr. Baker entirely fails to understand my argument when he quotes what he thinks is my explanation. I am concerned to lay stress on the importance of variation in adult sex-ratio as a definite index of population tendency, and to show how sex ratios may be influenced by miscegenation; consequently, variations in differential sex-survival play an important part. I am less concerned to argue whether high sex-ratio is a contributory cause or the effect of decline in population, but I think I have proved that they are correlated. It is also suggestive that a high sex-ratio, with an excess of men over women, must tend to diminish the monandrous quota of women and increase the polyandrous quota, and that polyandry is unfavourable to fertility.

It is, of course, no objection to, but supports my theory to say that monandry if often correlated with an increasing population, but to make the same statement when the word monogamy is substituted for monandry is to deal with fictions. If by monogamy we imply a general monandrous monogyny throughout the ethnic group examined as the only type of mating, I know of no evidence that it has ever anywhere existed in the history of humanity. It is also true that polygyny favours the segregation of women during pregnancy and lactation. It mitigates or avoids the consequences of overgestation; one of the most important consequences among primitive people being the malnutrition of infants due to their mothers' next pregnancy. The consequences of what the Fijians called *dabe* was well recognised by competent observers.

As a final point, it may be observed that the theory of Colonel Marshall to account for the high sex-ratio at birth of the Todas is not necessarily as untenable as Mr. Baker supposes. Marshall simply supposed that a high birth sex-ratio was an inheritable tendency, and that by selection male-producing families might increase in the population. The often quoted experiment undertaken by Helen King showed that by selection from the same fairly homozygous stock of rats having a normal sex ratio of 105 males to 100 females, two strains were produced with two contrasted sex-ratios. After nineteen generations one strain had a high sex-ratio of 122.3 males to 100 females and the other a low sex-ratio of 81.8.

G. PITT-RIVERS.

Games.

Hornblower.

#### Indian Wrestling.

To the Editor of MAN.

SIR,—Captain Gordon's completion (MAN, 1928, 146) of the omissions in *The Times* article on Indian wrestling

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(not mine) is of great interest. He does not say where the wrestlers "go up" after the bouts to make their final salute, nor what is "the presiding spirit of the arena," on which points and others concerning the ritual, or perhaps sacred, aspect of wrestling information would be valuable.

In Egypt, besides the actions reported by me there is no trace of any such aspect: wrestling there has nothing of the importance given to it in India and is but one show among many. In Indo-European races, on the other hand, it held in past ages a solemn place among national and civic practices, as may readily be seen in tracing its history in ancient Greece, where it is connected with the names of gods, demigods and heroes—especially founder-heroes, such as Poseidon, Herakles and Theseus. But in ancient Egypt the wrestling so fully illustrated on tomb-walls of the Middle Kingdom appears to be but training exercise for soldiers and I do not think that any traces of solemn wrestling can be found. It seems likely that the ritual practices witnessed by me were taken over by Egyptian wrestlers from Indian ones, with modifications to suit

their religion, Islam. The whole matter is of interest and well worth the attention of students.

Yours faithfully,

G. D. HORNBLOWER.

Ethnology.

Thomas.

### Children of the Sun.

To the Editor of MAN.

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SIR,—With reference to Mr. Perry's letter (MAN, 1928, 122) regarding the instances of "independent invention" adduced by Dr. Hutton (MAN, 1928, 65). When I (who, it should be said, am not Mr. N. W. Thomas) "swung bits of wood" on the end of a string, it did not stop at that; I noticed the sound and was impressed by it. I noticed that the thing whistled, hummed, or boomed according to the size of the piece of wood. I lived, like most boys and all primitive peoples, in an atmosphere of enquiry (v. Mr. Perry's para. 5). Had I been a savage, and my discovery absolutely novel to my community, I venture to think the complicated ritual which Mr. Perry mentions as bound up with the bull-roarer might easily have developed in time on lines similar enough to be regarded (at their face value) by anthropologists as due to diffusion.

Yours, etc.,

E. S. THOMAS.

## ANTHROPOLOGICAL NOTES.

Archæological Notes in "Antiquity": 158  
some Replies.

We have received from Mr. J. P. T. Burchell an open letter relating to the Editorial Notes which appeared in *Antiquity* in October last, referring to the Sligo finds as "certain limestone boulders which are claimed as humanly made objects." Mr. Burchell states that Mr. Crawford, the Editor of *Antiquity*, after examining the "boulders," was of the opinion that they represented the work of man. Mr. Burchell states that he is exonerated from an undertaking not to make this opinion public by the fact that the Editor of *Antiquity* has referred to the Sligo finds in his columns in a misleading manner, "deliberately darkening council."

In the same issue of *Antiquity* it was stated that immense numbers of rostro-carinate flint implements had been found on certain sites. Mr. Reid Moir writes to point out that this type of artifact is of considerable rarity, but that even if the statement were true, as it is in the case of palæolithic hand-axes in certain deposits, it would be no argument against their human origin. This can only be determined by an enlightened examination of the flake scars observable upon the stones themselves. As regards numbers, he points out that Baldwin Spencer records the production of something like

80,000 humanly flaked pieces of stone by a small party of Australian blacks in one week.

Mr. Moir goes on to point out that the suggestion that the deposits in East Anglia in which he has found, for instance, the sub-red crag implements, are disturbed, is unfounded and that no competent person who has examined the sites disputes the fact that the beds are *in situ* and undisturbed. The same applies to other sections where other and later implements have been unearthed. The fact that human bones have not been found beneath the red crag has no relation to the question whether the implements of that horizon are naturally or artificially produced. Their absence affords no criterion of the genuineness of, say, Acheulean specimens.

As regards the facts about the Ipswich skeleton, which Mr. Reid Moir fully acknowledges, he refers his critics to his recently published book "The Antiquity of Man in East Anglia."

Mr. Reid Moir wished to reply as soon as possible to the criticisms in the pages of *Antiquity*, but, unfortunately, the following number of that journal had already been made up. This must be the excuse for referring in MAN to a matter which concerns another periodical but is at the same time of interest to all archæologists.













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